How Conditional Cash Transfers Impact Schooling and Work for Children and Youth in Urban Mexico[1]

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February, 2010

[1]This paper builds on a paper that Behrman, Gallardo-García, Parker and Todd wrote in 2006 as consultants to the Instituto Nacional de Salud Pública's (INSP) ongoing evaluation of Oportunidades under a subcontract to Todd (PI) , with additional support from the Mellon Foundation/Population Studies Center (PSC)/University of Pennsylvania grant to Todd (PI). Jere Behrman is W.R. Kenan Jr. Professor of Economics and a PSC Research Associate at the University of Pennsylvania. Parker is a Profesora/Investigadora, División de Economía, CIDE, Mexico City. Todd is a Professor of Economics and a PSC Research Associate at the University of Pennsylvania. Jorge Gallardo-García is a manager at Bates-White consulting firm and Viviana Vèlez-Grajales is an associate of the Inter-American Development Bank.

Abstract

Conditional cash transfer programs have spread worldwide. Many papers have studied their impacts on schooling in rural areas. However, little is known about their impacts in urban settings. This paper studies the short-term (one and two year) effects of the Mexican *Oportunidades* conditional cash transfer program on urban children/youth. The program provides financial incentives for children/youth to attend school and for family members to visit health clinics. To participate, poor families had to sign up for the program and be deemed eligible. Difference-in-difference propensity score matching estimates indicate statistically significant program impacts on school enrollment, schooling attainment, time devoted to homework, whether parents help on homework and on working.

1 Introduction

Conditional Cash Transfer (CCT) programs have spread quickly internationally as a new form of social assistance for poor populations.[1] The programs aim to lessen current poverty and to promote human development by stimulating investments in health, education and nutrition, particularly over the early stages of the life cycle. CCT programs provide cash incentives for families to engage in certain behaviors, such as sending children to school and/or attending health clinics for regular check-ups, prenatal care and to receive infant nutritional supplementation. One of the earliest and largest scale CCT programs is the Mexican *Oportunidades* program (formerly called PROGRESA), in which 30 million people are currently participating. Cash benefits are linked both to regular school attendance of children and to regular clinic visits by beneficiary families. Average benefits amount to about US35 to 40 dollars monthly.

The *Oportunidades* program was first initiated in 1997 in rural areas of Mexico. Impact evaluations based on a randomized social experiment carried out in rural villages demonstrated significant program impacts on increasing schooling enrollment and attainment, reducing child labor and improving health outcomes.[2] In 2001, the program began to expand at a rapid rate into semiurban and urban areas. Impacts in urban areas might be expected to differ from those in rural areas, both because some features of the program have changed and because access to schooling and health facilities as well as work opportunities differ. Although there have been a number of evaluations of the schooling impacts of *Oportunidades* and other CCT programs in rural areas, relatively little is known about their effectiveness in urban areas. Evaluations of CCT programs in other Latin American countries in rural areas have shown overall similar impacts to those in rural Mexico (Parker, Rubalcava, and Teruel, 2008).

[1] Programs are currently available or under consideration in over 30 countries, primarily in Latin America but also in Asia, Africa and developed countries (e.g., New York City) and have been advocated by international organizations such as the Inter-American Development Bank and the World Bank. See Becker (1999) for an early discussion of how CCT programs reduce the use of child labor. See Fiszbein et. al. (2009) for a recent survey of CCT programs that have been implemented in a range of low and middle income countries around the world.

[2] See, e.g., Schultz (2000, 2004), Behrman, Sengupta and Todd (2005), Parker and Skoufias (2000), Skoufias, E. and B. McClafferty (2001), Todd and Wolpin (2006), Behrman, Parker and Todd (2009), Parker and Behrman (2008) and Parker, Todd and Wolpin (2009). Also, see Buddelmeyer and Skoufias (2003) and Diez and Handa (2006) for nonexperimental approaches to evaluating the program in rural areas and for comparisons of nonexperimentally and experimentally obtained impact estimates, which are found to be similar.

Although the structure of benefits is identical in rural and urban areas, the procedure by which families become beneficiaries differs. In the rural program, a census of the targeted localities was conducted and all families that met the eligibility criteria for the program were informed of their eligibility status. For cost reasons, this type of census could not be carried out in urban areas, and an alternative system of sign-up offices was adopted. The location of the sign-up offices was advertised and households that considered themselves potentially eligible had to visit the offices to apply for the program during a two month time period when the offices were open. About 40% of households who were eligible did not apply for the program, which contrasts sharply with the very high participation rates that were observed in rural areas. About one third of nonparticipating but eligible, urban households report not being aware of the program. As described below, our program impact estimation strategy uses the information on eligible nonparticipating households from intervention areas to estimate a model of program participation that depends on characteristics of the household and of the locality in which they live. Another important difference between the initial rural program and the current program is that the schooling subsidies were initially given for attendance in grades 3 through 9, but were extended in 2001 to grades 10 to 12 (high school). We might therefore expect to see impacts of the program on older children as well as potentially larger impacts on younger children than observed under the original rural program.

This paper evaluates the effectiveness of *Oportunidades* in influencing schooling and working behaviors of adolescent youth using observational data. Specifically, it examines how participation in the proram for two years affects schooling attainment levels, school enrollment rates, time devoted to homework, working rates and monthly wages. Our analysis focuses on girls and boys who were 6-20 years old in 2002, when the *Oportunidades* program was introduced in some urban areas. The treated group are households who were eligible for the program and elected to participate during the two month incorporation window. The comparison group consists of eligible households living in localities where the program was not yet made available. These localities were preselected to be otherwise similar to the treatment localities in terms of observable average demographics and in terms of other characteristics, such as regional location, availability of schools and health clinics. Nonetheless, in making comparisons between individuals in the treated and comparison

groups, it is important to account for program self-selection to avoid the potential for selection bias. We find that participating households differ from nonparticipating households; for example, households who expect a larger gain from participation in terms of subsidies are more likely to seek incorporation. The methodology we use to take into account nonrandom program selection is difference-in-difference matching (DIDM). Matching methods are widely used in both statistics and economics to evaluate social programs.[3] The DIDM estimator compares the change in outcomes for individuals participating in the program to the change in outcomes for matched nonparticipants. The advantage of using a DIDM estimator instead of the more common cross-sectional matching estimator is that the DIDM estimator allows for time-invariant unobservable differences between the participant and nonparticipant groups that may affect participation decisions and outcomes. Such differences might plausibly arise, for example, from variation across regional markets in earnings opportunities. We also report impact estimates based on a standard difference-in-difference estimator.

This paper makes use of data on three groups: (a) households who participated in the program, (b) households who did not participate but were eligible for the program and lived in intervention areas, and (c) households living in areas where the program was not available but who otherwise met the program eligibility criteria. Groups (a) and (b) are used to estimate a model of the program participation decision, while groups (a) and (c) are used for impact evaluation. Importantly, group (b) is not used as a comparison group for the purposes of impact evaluation because of the potential for spillover effects of the program onto nonparticipating households in intervention areas. For example, if the school subsidy component of the program draws large numbers of children participating in the program into school, then it can lead to increased pupil-teacher ratios that represent a deterioration in school quality for children who are not directly participating.[4] By drawing the comparison group instead from geographic areas that had not yet received the intervention and assuming that the same program participation model applies to those areas, matching estimators

[3] See, e.g. Rosenbaum and Rubin (1992), Heckman, Ichimura and Todd (1997), Lechner (2002), Jalan and Ravaillon (2003), Behrman, Cheng and Todd (2004), Smith and Todd (2005), Galiani, Gertler, and Schargrodsky (2005).

[4] For evidence of spillovers in the rural program, see Angelucci and De Giorgi (2009), Bobonis and Finan (2009) and Angelucci et. al. (2010)

can still be applied.[5] Specifically, we apply the participation model estimated from groups (a) and (b) to predict which of the households in (c) would most likely participate were the program made available.

Our analysis samples come from the Urban Evaluation Survey data that were gathered in three rounds. One round is baseline data, gathered in the fall of 2002 prior to beneficiary households beginning to receive program benefits. The 2002 survey gathered information pertaining to the 2001-2002 school year. The other two rounds were gathered post-program initiation in the fall of 2003 and fall of 2004 after participating households had experienced one and two years in the program and pertain to the 2002-2003 and 2003-2004 school years. In all rounds, data were gathered on households living in both intervention and nonintervention areas.

The paper proceeds as follows. Section 2 briefly summarizes the main features of the Oportunidades program. Section 3 describes the parameters of interest and the estimation approach. Section 4 discusses the data and presents simple mean comparisons across the various analysis samples. Section 5 describes how we estimate the model for program participation. Section 6 presents estimated program impacts on education and work and Section 7 concludes.

2 Description of the *Oportunidades* Program

The *Oportunidades* program is targeted at families who meet the eligibility criteria, measured by a marginality index summarizing characteristics of the households, such as education, type of housing and assets. Households with index scores below a cutoff point are eligible to participate. The program has two main subsidy components: a health and nutrition subsidy and a schooling subsidy. To receive the health and nutrition benefit, household members have to attend clinics for regular check-ups and attend informational health talks. To receive the school subsidy, children or youth in participating households have to attend school in one of the subsidy-eligible grade levels (grades 3-12) for at least 85% of school days. They also cannot receive a subsidy more than twice for the same grade.

[5] Angelucci and Attanasio (2009) discuss similar concerns in their study of *Oportunidades'* impact on urban consumption.

Because of these conditionalities, one would expect to find positive program impacts on school enrollment and schooling attainment. However, the magnitude of these impacts is not predictable, because households can participate to different degrees in the program. For example, households can choose to participate only in the health and nutrition component, so it is possible that the program has no impact on schooling for some households. Second, participating households can choose to send only a subset of their children to school for the required time. For example, rather than send two children to school fifty percent of the time, households might send their older child to school 85% of the time (to receive the subsidy) and keep the other child at home. Thus, children within the household may be affected by the program in different ways. The goal of this paper is to assess the magnitude of the *Oportunidades* impacts on a variety of school and work-related outcome measures. Some of these outcomes, such as school enrollment, schooling attainment and grade accumulation, are closely tied to program coresponsibilies, while other outcomes, such as time spent doing homework, whether parents help with schoolwork, and employment and earnings, are not.[6]

Table 1 shows the subsidy amounts and how they increase with grade level, a design that attempts to offset the higher opportunity costs of working for older children. The slightly higher subsidies for girls reflect one of the emphases of the program, which is to increase the schooling enrollment and schooling attainment for girls, who traditionally have lower enrollment in post-primary grade levels in Mexico.

3 Parameters of Interest and Estimation Approach

The general evaluation strategy used in this paper compares outcomes for program participants (the treatment group) to those of a comparison group that did not participate in the program. Data were gathered on households living in urban areas served by *Oportunidades* (intervention areas) and on households living in urban areas that had not yet been incorporated into *Oportunidades* (non intervention areas). Baseline data (year 2002) are available as well as two years of follow-up

[6]The estimates here represent the total impact of the program on schooling; we are unable to isolate, for instance, the impact of the grants linked to schooling from, say, potential health effects on schooling from the regular clinic visits.

data (year 2003 and 2004). We distinguish among the following three groups of households:

(a) Participant Group - Households that applied to the program, were deemed eligible for it and chose to participate.

(b) Eligible Non-Participant Group - Households living in program intervention areas that did not apply to the program but were eligible for it.

(c) Outside Comparison Group – Households living in localities where *Oportunidades* was not yet available but who otherwise meet the eligibility criteria for the program. These localities were planned for incorporation after the 2004 data collection, but the households living in these localities did not know whether and when they would be incorporated. We refer to group (a) as the treatment group and groups (b) and (c) as the comparison groups.

The main parameter of interest in this paper is the *average impact of treatment on the treated*, for various subgroups of children/youth defined by age, gender and preprogram poverty status. Let Y_1 denote the potential outcome of a child/youth from a household that participates in the program and let Y_0 denote the potential outcome if not exposed to the program. The treatment effect for an individual is:

$$\Delta = Y_1 - Y_0.$$

Because no individual can be both treated and untreated at the same time, the treatment effect is never directly observed. This paper uses matching methods to solve this missing data problem and focusses on recovering mean program impacts rather than the full distribution of impacts.[7]

Let X denote a set of conditioning variables and let $D = 1$ if an individual is from a participating household, $D = 0$ if not. The average impact of treatment on the treated parameter is:

$$\begin{aligned} E(\Delta|D &= 1) = E(Y_1 - Y_0|X, D = 1) \\ &= E(Y_1|X, D = 1) - E(Y_0|X, D = 0) \end{aligned}$$

We can use the treatment data to estimate $E(Y_1|X, D = 1)$, but the data required to estimate $E(Y_0|X, D = 1)$ are missing.

[7] See Heckman, Lalonde and Smith (1999) for a discussion of other possible parameters of interest in evaluating effects of program interventions.

Estimators developed in the evaluation literature provide different ways of imputing $E(Y_0|X, D = 1)$. Traditional matching estimators pair each program participant with an observably similar nonparticipant and interpret the difference in the outcomes of the matched pairs as the treatment effect (see, e.g., Rosenbaum and Rubin, 1983). Matching estimators are typically justified under the assumption that the outcome is independent of program participation conditional on a set of observable characteristics. The assumption is that there exists a set of observable conditioning variables Z for which the nonparticipation outcome Y_0 is independent of treatment status D conditional on Z:

$$Y_0 \perp\!\!\!\perp D|Z. \tag{1}$$

It is also assumed that for all Z there is a positive probability of either participating $(D = 1)$ or not participating $(D = 0)$, i.e.

$$0 < \Pr(D = 1|Z) < 1, \tag{2}$$

which implies that a match can be found for all treated $(D = 1)$ persons. Under these assumptions, after conditioning on Z, the Y_0 distribution observed for the matched nonparticipant group can be substituted for the missing Y_0 distribution for participants.[8] If there are some observations for which $\Pr(D = 1|Z) = 1$ or 0, then it is not possible to find matches for them.

Matching methods can be difficult to implement when the set of conditioning variables Z is large.[9] To address this difficulty, Rosenbaum and Rubin (1983) provide a useful theorem that shows that when matching on X is valid, then matching on the propensity score $\Pr(D = 1|Z)$ is also valid. Specifically, they show that assumption (1) implies

$$Y_0 \perp\!\!\!\perp D|\Pr(D = 1|Z).$$

Provided that the propensity score can be estimated at a rate faster than the nonparametric rate

[8] If the parameter of interest is the mean impact of treatment on the treated (TT), then (1) and (2) can be replaced by weaker assumptions:

$$E(Y_0|Z, D = 1) = E(Y_0|Z, D = 0) = E(Y_0|Z),$$

and

$$\Pr(D = 1|Z) < 1.$$

See Heckman, Ichimura and Todd (1997) for discussion on this point.

[9] If X is discrete, small (or empty) cell problems may arise. If X is continuous, and the conditional mean $E(Y_0|D = 0, X)$ is estimated nonparametrically, then convergence rates will be slow due to the "curse-of-dimensionality."

(e.g. parametrically), the dimensionality of the matching problem is reduced by matching on the univariate propensity score. For this reason, much of the recent evaluation literature focuses on propensity score-based methods.[10]

As described above, cross-sectional matching estimators assume that mean outcomes are conditionally independent of program participation. However, for a variety of reasons, there may be systematic differences between participant and nonparticipant outcomes that remain even after conditioning on observables. Such differences may arise, for example, because of program selectivity on unmeasured characteristics, or because of level differences in outcomes across different geographic locations in which the participants and nonparticipants reside.

A difference-in-differences (DIDM) matching strategy, as proposed and implemented in Heckman, Ichimura and Todd (1997), allows for temporally invariant differences in outcomes between participants and nonparticipants and is therefore less restrictive than cross-sectional matching. [11] It is analogous to the standard DID regression estimator but does not impose the linear functional form restriction in estimating the conditional expectation of the outcome variable and reweights the observations according to the weighting functions used by the matching estimator. Letting t' denote a pre-program time period and t a post-program time period, the DIDM estimator assumes that

$$Y_{0t} - Y_{0t'} \perp\!\!\!\perp D | \Pr(D = 1|Z),$$

or the weaker condition

$$E(Y_{0t} - Y_{0t'}|D = 1, Z) = E(Y_{0t} - Y_{0t'}|D = 0, Z)$$

along with assumption (2) (the support condition), which must now hold in both periods t and t' (a non-trivial assumption given the attrition present in many panel data sets). The local linear DIDM estimator that we apply in this paper is given by

$$\hat{\alpha}_{DD} = \frac{1}{n_1} \sum_{i \in I_1 \cap S_P} (Y_{1ti} - Y_{0t'i})$$

[10] Heckman, Ichimura and Todd (1998), Hahn (1998) and Angrist and Hahn (2001) consider whether it is better in terms of efficiency to match on $P(X)$ or on X directly. For the TT parameter considered in this paper, neither is necessarily more efficient than the other (see discussion in Heckman, Ichimura and Todd (1998)).

[11] Smith and Todd (2005) find better performance for DIDM estimators than cross-sectional matching estimators in an evaluation of the U.S. National Supported Work Program.

$$- \sum_{j \in I_0 \cap S_P} W(i,j)(Y_{0tj} - Y_{0t'j}),$$

where I_1 denotes the set of program participants, I_0 the set of non-participants, S_P the region of common support and n_1 the number of persons in the set $I_1 \cap S_P$. The match for each participant $i \in I_1 \cap S_P$ is constructed as a weighted average over the difference in outcomes of multiple non-participants, where the weights $W(i,j)$ depend on the distance between P_i and P_j. Appendix A gives the expression for the local linear regression weights and also describes the method that we use to impose the common support requirement.[12]

Estimating the propensity score $P(Z)$ requires first choosing a set of conditioning variables, Z. It is important to restrict the choice of Z variables to variables that are not influenced by the program, because the method assumes that the distribution of the Z variables is invariant to treatment. As described below, we take the set of Z variables to be baseline characteristics of persons or households that are measured prior to the introduction of the program and estimate the propensity scores by logistic regression.

Lastly, because the propensity score model is estimated from groups (a) and (b) and then applied to groups (a) and (c) to determine the impacts, our matching procedure assumes that the model for program participation decisions is the same for intervention areas as in nonintervention areas but allows for households in nonintervention localities to have a different distribution of observed characteristics Z. [13]

4 Data Subsamples

We use the Urban Evaluation Survey (Encelurb) baseline carried out in the fall of 2002 and two follow-up rounds carried out in the fall of 2003 and 2004. The baseline was carried out before any monetary benefits had been received and collected information pertaining to the 2001-2002

[12] Research has demonstrated advantages of local linear estimation over simple kernel regression estimation. These advantages include a faster rate of convergence near boundary points and greater robustness to different data design densities. See Fan (1992a,b). Local linear regression matching was applied in Heckman, Ichimura and Todd (1998), Heckman, Ichimura, Smith and Todd (1998) and Smith and Todd (2005). We do not report results based on nearest neighbor matching estimators, in part because Abadie and Imbens (2005) have shown that bootstrap standard errors are inconsistent for nearest neighbor matching estimators. Their critique does not apply to the local linear matching estimators used here.

[13] The assumption that the participation model is the same across intervention and nonintervention areas is untestable, as we cannot observe participation decisions in nonintervention areas.

school year. The Encelurb data include rich information on schooling and labor force participation, as well as information on income, consumption, assets and expectations about future earnings. The database contains information on 2972 households in the eligible participant group (group a), 2556 households who were eligible but did not participate (group b) and 3607 households from nonintervention areas who satisfied the eligibility criteria for the program (group c).

The intervention areas were selected by first choosing a random sample of neighborhoods where the program was scheduled to be started in 2002. Then, a stratified random sample of 149 blocks was chosen with all households in the blocks included in the sample. The comparison group blocks were chosen by matching localities on average characteristics based on pre-program locality-level data from the 2000 Census. Program administrative data were used to define who received benefits and who did not.

As described in Appendix B, some observations had to be eliminated from our analysis because of inconsistencies in reported ages and schooling levels across the survey rounds. Appendix B details how we constructed variables for our analysis and how we identified and dealt with different kinds of data inconsistencies. Table 2 shows the sample sizes for groups (a), (b) and (c), comprised of boys and girls age 6-20 in 2002 (age 8-22 in 2004). These are the sample sizes prior to deleting observations for reasons of attrition across rounds and missing data. For details on how the final analysis samples were obtained, see Appendix B.

4.1 Interpretation of the estimator in the presence of attrition

In this study, we focus on children and youth that were observed across all the years of the survey. Table 3 provides information on the number of children in groups (a) and (c) (the groups used for the impact analysis) by age, survey round and control versus treatment groups. The percentage of children who are lost between rounds increases with age from 18% for 6-14 olds and to 36% for the oldest age category (19-20 year olds) for 2002-2004. The higher attrition rate for the oldest group is not surprising given that children typically leave home and get married at earlier ages in Mexico than in higher per capita income countries. Of more relevance for our analysis is whether the patterns differ substantially between the treatment and the control groups. Between 2002 and

2004 there are slightly higher attrition rates for most age groups (except for 15-18 year olds) and on average for the control (22%) than the treatment (19%). But the overall difference (3%) and the differences for each of the age groups (-1% to 4%) are fairly small, and therefore not likely to affect substantially the estimates presented below. Nevertheless, we describe below the extent to which our estimation method accommodates attrition.

As described in the previous section, our impact analysis aims to recover the average effect of treatment on the treated. In our application, the treated correspond to children observed over the three survey rounds. For the DIDM estimator to be valid in the presence of some attrition, we require an additional assumption on the attrition process. Let $A = 1$ if the individual attrited between time t' and t, else $A = 0$ and, for ease of notation, denote $\Delta Y = Y_{0t} - Y_{0t'}$. By Bayes rule,

$$\begin{aligned}\Pr(D = 1|A = 0, \Delta Y, Z) &= \frac{\Pr(A = 0|D = 1, \Delta Y, Z)\Pr(D = 1|\Delta Y, Z)}{\Pr(A = 0|\Delta Y, Z)} \\ &= \frac{\Pr(A = 0|D = 1, \Delta Y, Z)\Pr(D = 1|\Delta Y, Z)}{\Pr(D = 1|\Delta Y, Z)\Pr(A = 0|\Delta Y, Z, D = 1) + \Pr(D = 0|\Delta Y, Z)\Pr(A = 0|\Delta Y, Z, D = 0)}\end{aligned}$$

Under the DIDM assumption,

$$\Pr(D = 1|\Delta Y, Z) = \Pr(D = 1|Z).$$

We invoke an additional assumption that attrition is not selective on the change in potential outcomes in the no-treatment state conditional on Z and D, then

$$\Pr(A = 0|\Delta Y, D, Z) = \Pr(A = 0|D, Z)$$

we get

$$\Pr(D = 1|\Delta Y, Z, A = 0) = \frac{\Pr(A = 0|D = 1, Z)\Pr(D = 1|Z)}{\Pr(A = 0|Z)} = \Pr(D = 1|Z, A = 0),$$

which would imply that the DIDM estimator is valid for nonattritors. Attrition is allowed to depend on time-invariant unobservables that are eliminated by the differencing, so attrition on unobservables is permitted.[14] However, we do have to rule out certain types of attrition, for

[14] Also, attrition can be selective to some extent on treatment outcomes because it can depend on Y_{1t}. For example, those who experience the lowest Y_{1t} could be the ones who attrit. In that case, the DIDM estimator would still be valid for the subgroup of nonattritors, but the average treatment effect for nonattritors would not generally be representative of the average treatment effect for the population that started in the program.

example, that individuals with particularly high or low differences in potential earnings or schooling outcomes (that are not explainable by the observed characteristics Z) are the ones who attrit. Also, the application of the DID matching estimator does not require assuming that attrition is symmetric in the treatment and comparison groups. The method would allow, for example, for the case of no attrition in the treated group and substantial attrition in the comparison group, as long as the attrition in the comparison group is not selective on ΔY, conditional on Z.

4.2 Descriptive analysis and comparison of groups at baseline

In addition to the usual school enrollment indicators, the Encelurb includes information on time spent doing homework. The labor module includes characteristics of both the primary and secondary job of all individuals over age 5 for the previous 12 months and, for the household head and spouse, information covering labor history for the past 24 months. Our empirical analysis focuses on schooling attainment , school enrollment, homework time, employment, and monthly earnings.

Figures 1a,b to 6a,b depict the average outcome variables by age and gender of the child/youth for the participant group (a) and the eligible nonintervention group (c). The outcome variables are measured prior to the program intervention, so they are informative about pre-program group differences and not about impacts of the program. Figure 1a graphs the schooling attainment of boys. For both the participant and nonintervention group, schooling attainment increases steadily with age until around age 15 when the rate of increase slows down. Schooling attainment reaches an average of 8 grades at age 20. The nonintervention group has slightly higher schooling attainment levels at each age until about age 14, after which the averages by group are roughly the same. For girls (Figure 1b), the pattern of increase in schooling attainment by age is similar to that of boys, with the comparison group having higher schooling attainment levels for all of the ages.

The fact that the schooling attainment levels are on average different between groups (a) and (c) in 2002, prior to implementation of the program, shows the importance of taking into account preprogram differences in evaluating program impacts. The DIDM estimator compares the change in schooling attainment for children in group (a), whose families participated in the program, to the change in schooling attainment for children from matched families in group (c).

Figures 2a and 2b graph school enrollment rates in the 2001-2002 baseline school year for the same groups. For both the participant and the nonintervention groups, enrollment rates are near 100% over the age range associated with primary school. After that, enrollment rates steadily decline. For girls, the enrollment rates of participants are lower than those of the nonintervention group over most of the age range, but for boys the enrollment rate for participants is higher at ages older than 12. Our impact evaluation compares the change in enrollment rates (post-program minus pre-program) for children in group (a) to the change in enrollment rates for matched children from the nonintervention group (c).

Figures 3a and 3b graph the percentage of children who receive help at home in doing their homework. The general pattern is that the percentage helped increases at first with age, peaks at about age 7-9 and then declines. For young children, age 6, the nonintervention group reports a higher percentage helped on homework, but for other ages the participants and the nonintervention group are fairly similar in terms of homework help patterns. Figures 4a and 4b show the number of weekly hours spent by boys and girls doing homework. The figures show that adolescent girls spend on average more time on homework than similar aged boys. A comparison of program participants and eligibles from the nonintervention group shows that they are fairly similar preprogram in terms of reported time spent on homework. Figures 5a and 5b compare the employment rates of boys and girls age 12-20 in the program participant and eligible nonintervention group at baseline. A child is defined as employed if he/she reports working for pay. The boys in the two groups report fairly similar rates of employment. However, for girls the employment rates are clearly higher in the participant than in the nonparticipant group. Lastly, Figures 6a and 6b exhibit the average income earned by boys and girls in the two groups. Children who are not working are included in the average as earning zero income. Participant girls have on average higher income than girls in the eligible nonintervention group, which is consistent with their higher rates of working.

5 Propensity Score Estimation

The propensity score DIDM method requires first estimating $\Pr(D = 1|Z)$, which we estimate using logistic regression applied to data on households who are eligible to participate and live in

areas where the program is available, that is groups (a) and (b) described in Section 2 of this paper. Participating in the program (D=1) means that a household member attended the module to sign up for the program (see Section 1) and that the household was deemed eligible and elected to participate. We use administrative data to determine which households are or are not participating.

An important question in implementing any evaluation estimator is how to choose the set of conditioning variables Z. We include in Z variables that are designed to capture key determinants of program participation decisions, such as measures of the benefits the family expects to receive from the program (based on the number of children in the family of various ages attending school), variables related to costs associated with signing up and participating in the program (such as poverty status of the family, employment status of family members, availability of schools and health clinics), and variables that potentially affect whether the family is aware of the program (such as geographic location, population living in the locality, average poverty score of people living in the locality, schooling of household members). All of the variables are measured prior to program initiation and are therefore unlikely to be influenced by the program. We chose the set of regressors to maximize the percentage of observations that are correctly classified under the model.[15]

Table B.1 in appendix B presents the coefficients of the program participation model estimation along with their standard errors. 73% of the observations are correctly classified under the estimated model as being either a participant or nonparticipant. In general, most of the coefficients are statistically significant at the 10 percent level. The results show that having a dirt floor or walls or ceilings made of provisional materials (cardboard, plastic, tires, etc.) increases the probability of participating in the program. Having piped water inside the house decreases the probability of participating, although this coefficient is small in magnitude. Having a truck or a refrigerator

[15]This model selection procedure is similar to that used in Heckman, Ichimura and Todd (1997). That paper found that including a larger set of conditioning variables in estimating the propensity score model improved the performance of the matching estimators. We do not use so-called balancing tests as a guide in choosing the propensity score model in part because of the very large number of regressors. Balancing tests cannot be used in choosing which variables to include in the model, (see the discussion of the limitations of such tests in Smith and Todd (2007)), but they are sometimes useful in selecting a particular model specification for the conditional probability of program participation, given a set of conditioning variables. For example, they provide guidance as to whether to interact some of the regressors. However, with the large number of variables included in our propensity score model, the set of possible interactions is huge.

also decreases the probability of participation in the program, which is consistent with these goods being indicators of higher economic status for a household.

Participating in some other in-kind subsidy program has a positive effect on the probability of participation. The other relevant programs operating under the Mexican social support system are the free tortilla program, the milk subsidy program, the free groceries program, and the free school breakfast program.[16]

The number of children of ages that make them eligible for attending primary or secondary school increases the probability of program participation, which is to be expected as their presence increases the household's potential benefits from the school enrollment transfers. Being a female headed household also increases the participation probability. Being poor, as classified by the *Oportunidades* poverty score, is associated with higher rates of program participation. Poorer households may be more likely to attend the sign-up offices, either because they live in high density poverty areas where information about the program is better disseminated or because they are more likely to believe that their application to get *Oportunidades* benefits will be successful.

Figures 7a and 7b show the histograms of the propensity scores that are derived for each household from the logit model estimates. The figures compare propensity scores for participant households (group a) to those for nonintervention households (group c). The histograms for both groups are similar. Most importantly, the figures reveal that there is no problem of nonoverlapping support. For each participant household, we can find close matches from the nonintervention group of households (as measured by similarity of the propensity scores).

6 Program Impacts

We next evaluate the impacts of participating in the program on schooling and work-related outcomes. Given the relatively high school enrollment levels for young children and lower levels for older children prior to the introduction of the program, we expect the impacts on school enrollment to be greater for older children. Because the program requires regular attendance, we expect to see

[16] Participating in the PROCAMPO program decreases the probability of participation, because this program focuses on rural areas and being eligible for this rural program would mean that the house is located far from the *Oportunidades* urban incorporation modules.

improvements in grade progression as measured by educational attainment. The school attendance requirements of the program are expected to increase the amount of time spent in school relative to work. Therefore, we expect the program to decrease the percentage of children/youth working, with a corresponding decrease in earnings. Our expectations for the one-year impact analysis are guided in part by the results of the short-term rural evaluation, which found no effects on primary school enrollment levels but statistically significant effects on increasing enrollment at secondary grades.[17]

As described previously, our evaluation strategy compares children/youth from households who participate in the program (group a) to observably similar (matched) persons from households living in the areas where the program was not yet available (group c). We impute propensity scores for the households in the nonintervention areas using the estimated logistic model, as described in Section 4 and Appendix B. For these households, the estimated propensity scores represent the probability of the household participating in the program had the program been made available in their locality. Because the propensity score model is estimated at the household level, all children within the same household receive the same score. However, our impact estimation procedure allows children of different gender and age groups from a given household to experience different program impacts.

Lastly, the program subsidies start at grade 3 (primary level), so 6 and 7 year-old children do not receive benefits for enrolling in school. However, we include these children in our impact analysis, because they may benefit from the subsidies received by older siblings and/or the expectation of cash payments at higher grades could provide an incentive to send younger children to school, as was found in rural areas (Behrman, Sengupta and Todd 2005).

6.1 Schooling Attainment

Tables 4a and 4b show the estimated program impacts on total grades of schooling attainment for boys and girls who are age 6 to 20 at the time of the 2002 survey (8-22 at the time of the 2004 survey). The columns labeled "Avg Trt 2002" and "Avg Con 2002" give the average schooling attainment

[17] See Behrman, Sengupta and Todd (2005), Parker, Behrman and Todd (2004), Parker and Skoufias (2004), and Schultz (2000).

at baseline (year 2002) for children in treatment and comparison households, respectively, and show that controls had slightly higher grades of attainment in that year. The program can achieve increases in grades of schooling attainment through earlier matriculation into school, lower rates of grade repetition and lower rates of dropping out. The columns labeled "1 Year DID Match" and "2 Year DID Match" give the one-year and two-year program impact estimates obtained through DID matching, where we match on propensity scores after exact matching by age and gender. Also, we do not impose any restriction on the estimation that the two year impacts exceed the one-year impact. The program might over the short run increase school attendance and educational attainment, but over the longer run might lead to lower attendance at older ages if youth attain their targeted schooling levels at earlier ages. For example, suppose that before the program, a child's targeted schooling level was ninth grade and it took eleven years to complete ninth grade (because some grades were repeated). The program might encourage the child to attend school more regularly and to increase the targeted schooling level from ninth to tenth grade. If as a result of the program the child completes ten grades in ten years as opposed to nine grades in eleven years, then the child finishes school at an earlier age with more grades completed. In this sense, schooling attainment could increase without necessarily increasing enrollment at older ages.[18]

The columns labeled "1 Year DID" and "2 Year DID" give impact estimates obtained using an unadjusted DID estimator where as the next two columns contain the DID matching estimates. The one-year impacts correspond to the 2003-2004 school year and two-year impacts to the 2004-2005 school year. As seen in the columns labeled "% Obs in Support," the supports for the most part overlap and not many of the treated observations were eliminated from the analysis for support reasons. Each of the tables presents impact estimates obtained using three alternative choices for the bandwidth (0.2, 0.3, and 0.4). The bandwidth controls how many control observations are used in constructing the matched outcomes. As the estimates may depend on bandwidth choice and there are a variety of criteria for choosing bandwidths, it is important to consider a range of choices.

For boys (Table 4a), there is strong evidence of an effect of the program on schooling attainment

[18]Behrman, Parker and Todd (2010) find for longer program exposure (up to six years) in rural areas that the impacts on schooling attainment are nondecreasing.

at ages 8-18, with the two year impact (2003-2004) being about 40% larger than the one year impact. For girls (Table 4b), one year and two year program impacts are statistically significant over ages 6-14. The two year impacts for girls are generally of similar magnitude to the one-year impacts and are somewhat lower than for boys, despite the higher subsidy rates for girls. For both girls and boys, the largest impacts are observed in the 12 to 14 age range (approximately 0.20 grades, which represents about a 4% increase in schooling attainment). The estimated impacts are largest for boys age 12 to 14, ranging from 0.23 to 0.30 grades. These ages coincide with the transition from primary to secondary school, when preprogram enrollment rates dropped substantially and when when the subsidy level increases substantially (See Table 1). The estimates do not appear to be that sensitive to the different bandwidth choices. Also, the DIDM and the simple DID estimates are often similar. The similarity of the estimates, however, does not imply that self-selection into the program is not an issue. As discussed in Heckman, Ichimura and Todd (1997), selection biases can sometimes fortuitously average to close to zero.

Tables 5a and 5b show the estimated impacts on school enrollment for boys and girls. The third and fourth columns (labeled Avg Trt in 20002 and Avg Con in 2002) give the enrollment rates of the children in the baseline 2001-2002 school year. The enrollment rates are usually over 90% for ages up to age 14, after which there is a significant drop. We observe statistically significant impacts of the program on enrollment rates mainly for boys and girls ages 6-14. Both the one-year and two-year impacts are larger for girls age 6-7 than for similar age boys. The impacts for the age 8-11 group are similar across boys and girls and are roughly of the same magnitude (around 2-3 percentage point increases in enrollment, which represents an increase of about 2-3%). The two-year impact is not necessarily larger than the one-year impact, but this is to be expected given that children are aging over the two-years and enrollment rates generally decline with age. Again, the estimates are relatively insensitive with respect to bandwidth choice.

Tables 6a and 6b examine the estimated impacts of the program on the percentage of children whose parents help them on their schoolwork, for the subsample of children reported to be in school at the baseline year. The potential impact of the program on parents helping with homework is theoretically difficult to predict. On the one hand, the subsidies may provide an incentive for

parents to help their children so that they pass grades levels according to the pace required under the program rules. On the other hand, if the subsidies enable children/youth to work less (outside of school) and spend more time on schoolwork, then children/youth may not require as much help. As children/youth increase school enrollment and decrease their time spent working at home or outside the home, parents may also decide to increase their time spent working and so may have less time available to help children on their schoolwork. As seen in Table 6a, for boys there are statistically significant impacts only at ages 8-11, when parents in households participating in the program have a rate of helping that is lower by around 10 percentage points in the first year and 12 percentage points in the second year. Similar negative impacts on homework help are observed for girls, who also show lower two-year impacts on homework help at ages 8-11, though the estimates for girls are for the most part statistically insignificantly different from zero. The general pattern of a decrease in help on homework is consistent with parents in participating households spending more time on work activities to substitute for their children working less (see discussion of employment impacts below).

Tables 7a and 7b examine the impacts of the program on the amount of time that boys and girls dedicate weekly to their studies, for the subsample of children reported to be in school in the 2001-2002 school year. For boys, the average number of hours devoted to homework at the baseline is increasing with age and is in the range of 5-8 hours. The largest one-year and two-year impacts of the program are observed for boys age 12-14 (in 2002), who devote an additional 0.85-1.3 hours to homework, which represents an increase of about 14- 21% over their baseline hours. We also observe statistically significant one-year impacts for boys age 6-7 and for boys age 8-11, who devote an additional 20 to 50 minutes per week on homework. For girls of all ages, however, the estimated two-year impacts are statistically insignificant.

Tables 8a and 8b show the estimated impacts of the program on the percentage of children working for pay. The tables only show estimates for ages 12-20, because very few children work at younger ages, probably in part because of legal restrictions on work for children younger than age 14. As seen in the third and fourth columns, showing the baseline work patterns, the percentage of children working for pay increases sharply with age. From age 12-14 to 15-18, the percentage

increases from 10% to 44% for boys and 10% to 27% for girls. Participation in the program is associated with a reduction in the percentage of boys age 12-14 working of about 8 percentage points in the first year and 12-14 percentage points in the second year. For girls, the percentage of youth who work is lower at all ages than for boys, and the program does not seem to affect the percentage of girls who work. The only statistically significant impact for girls is observed at ages 15-18, when the program lowers the percentage working by around 11%, but only in the first year. Thus the gender difference in working and in time spent on homework are consistent with significant shifts from work to homework for boys but no significant changes in either for girls.

Lastly, Tables 9a and 9b present estimates of the program's impact on monthly earnings of children and youth 12 to 20 years old, where earnings are set to zero for children who report not working. The average earnings combines information on the proportion working and on the average earnings of those who work. We observe an impact of the program on earnings only for boys age 12-14, for whom earnings decline in the first year after the program started. All of the other estimates are statistically insignificantly different from zero. This result is not necessarily inconsistent with the statistically significant effects on employment noted above, as it is possible that the decrease in the supply of child labor induced by the program increases the wages of the children who remain in the labor market.

7 Conclusions

This paper analyzes the one and two year impacts of participation in the *Oportunidades* program on schooling and work of children/youth who were age 6-20 in 2002 (8-22 in 2004) who live in urban areas and whose families were incorporated into *Oportunidades* program in 2002. One important difference between the implementation of the program in urban areas in comparison to rural areas is that families in urban areas had to sign up for the program at a centralized location during a time window of opportunity when the program office was open to them. Only a subset of the households eligible for the program chose to participate. Another important difference is that the data collection in urban areas is nonexperimental, and the estimators used to evaluate program impacts must take into account nonrandomness in the program participation process.

This paper makes use of data on three groups: (a) a group of eligible households who participate in the program, (b) a group of eligible households who did not participate but who lived in intervention areas, and (c) a group of eligible households who live in areas where the program was not available. The methodology used is the method of difference-in-difference matching, which compares the change in outcomes for individuals participating in the program with the change in outcomes for similar, matched individuals who are not in the program.

Our program impact analysis finds significant positive impacts for both boys and girls on schooling attainment, school enrollment, proportions working, and amount of time children spend doing homework. In addition, we find a statistically significant negative impact of the program on the percentage of children whose parents help them with their homework and no discernible impact of the program on average earnings.

Overall, the education impacts deriving from one and two years of program operation are sizeable. They are generally encouraging, given that the data on which they are based derives from the initial phase of expansion to urban areas, a time period where one might expect operational problems. [19] The impacts thus far are consistent with the program, at least in the area of education, being carried over to urban areas in a successful manner.

One concern in implementing the *Oportunidades* school subsidies in urban areas is that the level of subsidies in urban areas was identical to rural areas, but the opportunity costs of school for children/youth from urban areas are likely to be greater because of greater earnings potential. It is therefore remarkable that the impact estimates for the urban areas are for the most part similar to results found in the rural evaluation in the initial phases of the program. A difference, however, in the urban areas compared with the initial rural implementation is that school subsidies are now offered at grades 10-12, which provides incentives for older children to enroll in school and may also provide extra incentives for younger children to reach those grade levels.

With respect to gender, we found fairly similar impacts for boys and girls on schooling outcomes, which suggests that the slightly higher grants given to girls attending grades 7-12 are not generating large differences in impacts by gender (although one cannot say in the current analysis whether

[19] However, there may also be opposing pioneer effects as discussed in Behrman and King (2008) and King and Behrman (2009).

impacts on girls would be different if grant amounts were changed to be the same for girls as boys). Prior to the program, enrollment rates for girls in the participant groups started to fall at earlier ages than those for boys (See figures 2a and 2b), which could justify the higher grants to girls for secondary school grades. However, there do not appear to be strong differences in education levels between boys and girls (see figures 1a and 1b). In the evaluation of the short-term impacts in rural areas (Behrman, Sengupta and Todd, 2005), it was demonstrated that girls tend to progress more quickly than boys through the primary grades (i.e. have lower failure rates), which can explain why their completed average schooling is higher than that of boys despite leaving school on average at earlier ages.[20]

Boys report spending more time doing homework as a result of the program, so it is perhaps surprising that they seem to be receiving less help from parents/other relatives with the program than they would without the program. As described earlier, this may reflect either that they need less help or that other adults are working more in response to the program (perhaps as a response to their children working less) and thus have less time to spend during after school hours with their children. In any case, the impact of the program on the time allocation of different family members seems an important topic for further research.

In addition to program impacts on schooling, we find some evidence that the program affects youth working behavior. Boys age 12-14 in 2002 (14-16 in 2004) experience a significant decrease in working, ranging from 7-13 percentage points. Girls' work behavior is not significantly affected by the program, in part because a lower proportion of girls than boys work for pay.

We noted that there is little scope for an effect of the program on enrollment rates at primary grades when enrollment rates preprogram were already very high. If increasing average schooling levels is a primary aim of the program, then it is worth considering further whether decreasing or eliminating the subsidies at the lower grades and using the resources to increase the level of payments to higher grade levels would be a more effective program design.[21] Of course, changing

[20]This is a widespread pattern in developing countries. In all regions and almost all countries for which recent Demographic Health Surveys permit comparisons, though girls in some cases have lower enrollment rates, conditional on starting in school they have on average equal or higher schooling attainment (Grant and Behrman, 2010).

[21]Todd and Wolpin (2006) and Attanasio, Meghir and Santiago (2005) study how shifting resources towards higher grades would affect schooling attainment in their study of *Oportunidades* in rural areas.

the subsidy schedule in this way would have distributional consequences and would shift resources towards families whose children have higher schooling attainment levels. Further study of the urban data would be useful to evaluate the effectiveness of alternative program designs.

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Appendix A. Local Linear Regression and Method for Determining Common Support

This appendix gives expressions for the local linear regression weighting function (as introduced in Fan (1992a,b)) and also describes the method we use to impose the common support requirement of matching estimators. Letting $\tilde{P}_{ik} = P_k - P_i$, the local linear weighting function is given by

$$W(i,j) = \tag{3}$$

$$G_{ij} \sum_{k \in I_0} G_{ik} \tilde{P}_{ki}^2 - [G_{ij} \tilde{P}_{ji})][\sum_{k \in I_0} G_{ik} \tilde{P}_{ki}] \sum_{j \in I_0} G_{ij} \sum_{k \in I_0} G_{ij} \tilde{P}_{ki}^2 - \left(\sum_{k \in I_0} G_{ik} \tilde{P}_{ki} \right)^2 . \tag{4}$$

where $G(\cdot)$ is a kernel function and a_n is a bandwidth parameter.[22]

To implement the matching estimator given above, the region of common support S_P needs to be determined. By definition, the region of support includes only those values of P that have positive density within both the $D = 1$ and $D = 0$ distributions. Following the procedure in Heckman, Ichimura and Todd (1997), we require that the densities exceed zero by a threshold amount determined by a "trimming level" q. That is, after excluding P points for which the estimated density is exactly zero, we exclude an additional q percentage of the remaining P points for which the estimated density is positive but very low. The set of eligible matches are therefore given by

$$\hat{S}_P = \{P \in I_1 \cap \hat{S}_P : \hat{f}(P|D=1) > c_q and \hat{f}(P|D=0) > c_q\},$$

where c_q is the density cut-off trimming level.[23]

[22] We assume that $G(\cdot)$ integrates to one, has mean zero and that $a_n \to 0$ as $n \to \infty$ and $na_n \to \infty$. In estimation, we use the quartic kernel function, $G(s) = \frac{15}{16}(s^2 - 1)^2$ for $|s| \leq 1$,else $G(s) = 0$..

[23] The qth quantile c_q is determine by solving for

$$\sup_{c_q} \frac{1}{2J} \sum_{\{i \in I_1 \cap \hat{S}_P\}} \{1(\hat{f}(P|D=1) < c_q) + 1(\hat{f}(P|D=0) < c_q)\} \leq q,$$

where J is the number of observed values of P that lie in $I_1 \cap \hat{S}_P$. That is, matches are constructed only for the program participants for which the propensity scores lie in $\hat{S}_q$. The densities are estimated by the standard kernel density estimator, using Silverman's rule-of-thumb bandwidth.

Appendix B. Construction of Variables and the Propensity Score Model

Construction of Variables

In this appendix, we describe the construction of the main variables and the restrictions placed on the sample used in the analysis. It was necessary to exclude observations from the analysis that had missing data on key variables. The initial sample considered had 56,437 observations which included individuals from the groups used in the analysis mentioned before. Because the study is focused on the effects of the program on children's education, only individuals that were between 6 and 20 years old in 2002 were considered (34,161 individuals dropped). From those, 14,427 were in groups (a) or (c). Additional deletions were made on basis of age and other demographic information. For example, individuals with no age information for any of the rounds were deleted as well as individuals with information for all rounds but with age inconsistencies between rounds. Regarding the information on education, those individuals with no information in the baseline were not considered; and, individuals with contradictory information in the follow-up rounds with respect to grades completed and enrollment were not considered. These deletions left a usable sample of 12,160 individuals in group (a) or (c) in the baseline. Due to attrition, there is no information on some of those children in the data gathered in 2003 and 2004. From the 12,160 children, in the 2003 data we have information on 10,541 individuals and in 2004 we have 9,652 observations. Some additional observations were dropped in the impact analysis if they were missing the information required to obtain propensity scores or when information on the outcome variable was missing.

When the variable age was missing in all years, the individual was dropped from the sample. In the cases where the information was only available in 2003 or 2004 but not in 2002, the missing value was imputed using information from other years. The individual was dropped from the sample whenever the reported age difference from 2002 to any other years was negative or greater than 2 years in 2003 or greater than 3 in 2004, because the disparity is indicative of at least one age being reported with error.

The variable grades of schooling takes into account completed grades, not counting Kindergarten. We imposed an upper bound for the number of grades achieved in each degree according to the grades required for each degree by the SEP (the Ministry of Education in Mexico). For

example, an individual who reports having a primary degree is considered to have 6 (and no more) grades. Individuals that reported a value that exceeded the upper bound on the reported degree were imputed with the maximum number of grades, according to the degree reported.

The number of years for someone in secondary school degree was counted as 6 years of primary school plus the number of grades reported, up to a maximum of three grades. High school counted as 9 grades (primary plus secondary) plus the number of grades of high school reported, up to a maximum of three grades. The normal school degree is required to become a teacher in the national schooling system and the number of grades spent in normal school differs depending on the grades that the individual is going to teach. For this degree, we assume that finishing secondary level (12 grades of school) is the general prerequisite for normal school. The grades of education for someone with normal school sum to the grades of any previous degrees plus up to a maximum of three grades in normal school. The technical or commercial track has many varieties, making it difficult to assign the grades for individuals who report obtaining technical or commercial degrees. The grades of schooling for someone with a technical degree sum to the grades of any previous degrees (which were reported as prerequisites) plus the number of years of technical training, up to a maximum of three years. The college (university level) degree starts after high school so the number of grades is counted as 12 grades of school plus the number of grades of college reported, up to a maximum of 5 grades. Those reporting masters or doctoral degrees are assumed to have 17 plus the reported number of grades within the stated degree. Given these rules, an individual could have a schooling attainment of at most 20 grades.

Once the schooling attainment was determined for each individual in each year, we applied some additional restrictions with regard to consistency of the information. Because we assume that grades accumulate from first grade of primary and individuals usually start this degree when they are 6 years old, we imposed the restriction that the total grades of school attainment cannot be higher than the age of the individual plus one grade of school minus 5 (we used 5 instead of 6 to allow for the possibility that some individuals start grade 1 at an early age as in the data). For those exceeding the upper bound restriction, the grades of school attainment were considered as missing. Also, when the difference between completed school grades from 2002 to 2003 was

negative or greater than two the information was considered to be missing; this is, we did not allow for decreases in school grades or skipping more than one school grade in a natural year.

School enrollment status was determined by the question: Did you attend school last year? There is information for 2001-2002, 2002-2003 and 2003-2004 school years regarding this variable. We did not allow for discrepancies between school enrollment and number of school grades attained. Individuals that reported non-enrollment on the school year 2002 but reported an increase in school grades were excluded.

The variables pertaining to hours spent doing homework and whether the child receives help with homework were constructed conditional on attending school in the 2001-2002 school year. The variables are set to zero if the individual reports enrollment in the 2001-2002 school year but non-enrollment in the current school year.

An individual is considered employed in either year if reported to have worked the week before to being surveyed or to have a job but not have worked in the reference week.

The income variable used in our analysis is measured on a monthly basis. It includes the monthly income from both the principal job and any secondary job. Income is set to zero if an individual is not employed. Also, if individuals report income in weekly or annual terms, we convert the reported income to a monthly measure.

The Propensity Score Model

The propensity scores were estimated using a logistic model. A household was considered to be a participant or treated if it was located in an intervention area, was eligible and was incorporated by the program, according to the administrative data. A household was considered non-participant if it was located in an intervention area, was eligible but was not incorporated. Table B.1 presents the list of variables used in the logistic model, along with their definitions. Table B.2 shows the estimated coefficients. The key predictors of program participation are described in the text.

Table 1
Monthly amount of educational subsidies (pesos)*

Level/Grade	Boys			Girls		
	2002	2003	2004	2002	2003	2004
Primary						
3rd grade	100	105	110	100	105	110
4th grade	115	120	130	115	120	130
5th grade	150	155	165	150	155	165
6th grade	200	210	220	200	210	220
Secondary						
1st grade	290	305	320	310	320	340
2nd grade	310	320	340	340	355	375
3rd grade	325	335	360	375	390	415
High School						
1st grade	490	510	540	565	585	620
2nd grade	525	545	580	600	625	660
3rd grade	555	580	615	635	660	700

* Amounts in second semester of the year. 1US = 11pesos.

Table 2
Sample sizes for analysis sample

Sample	Number of Observations
Children/youth age 6-20 in 2002	
(A) from households participating in the program	5,819
(B) from eligible households living in intervention areas but do not participate	3,078
(C) from households who satisfy the program eligibility criteria but who live in nonintervention areas	6,341

Table 3
Attrition between 2002-2003 and 2003-2004

Age in 2002	Children in 2002	Children in 2003	% attrition	Children in 2004	%attrition
			Control Group		
6-7	1,198	1,040	13%	962	20%
8-11	2,570	2,212	14%	2,055	20%
12-14	1,334	1,147	14%	1,076	19%
15-18	979	806	18%	700	28%
19-20	260	195	25%	165	37%
6-20	6341	5,400	15%	4,958	22%
			Treatment Group		
6-7	983	888	10%	820	17%
8-11	2,238	2,039	9%	1,891	16%
12-14	1,276	1,149	10%	1,066	16%
15-18	1,034	848	18%	730	29%
19-20	288	217	25%	187	35%
6-20	5819	5,141	12%	4,694	19%

Table 4a

Estimated Impacts on Years of Schooling
Subgroup: Boys, Age 6-20
Local Linear matching †

Age in 2002	BW	Avg Trt in 2002	Avg Con in 2002	1 Year DID Match	2 Year DID Match	1 Year DID	2 Year DID	% Obs in Support ˜	% Obs in Support ˜˜	1 Year Obs T+C	2 Year Obs T+C
6-7	.2	0.69	0.90	0.02	-.03	0.06*	0.02	97	97	843	753
				(0.04)	(0.06)	(0.04)	(0.04)				
8-11	.2	3.08	3.44	0.11*	0.04	0.11*	0.06	97	97	1884	1712
				(0.03)	(0.03)	(0.02)	(0.03)				
12-14	.2	5.78	6.49	0.17*	0.24*	0.17*	0.23	97	97	1003	901
				(0.04)	(0.05)	(0.03)	(0.05)				
15-18	.2	7.60	7.67	0.10*	0.23*	0.11*	0.22	96	95	595	477
				(0.05)	(0.11)	(0.04)	(0.08)				
19-20	.2	7.81	8.01	0.04	0.02	0.03	-.06	96	96	145	106
				(0.07)	(0.22)	(0.04)	(0.10)				
6-20	.2	4.09	4.25	0.10*	0.10*	0.12*	0.11	97	97	4470	3949
				(0.02)	(0.03)	(0.01)	(0.02)				
6-7	.3	0.69	0.90	0.04	-.03	0.06*	0.02	97	97	843	753
				(0.04)	(0.06)	(0.04)	(0.04)				
8-11	.3	3.08	3.44	0.11*	0.05	0.11*	0.06	97	97	1884	1712
				(0.02)	(0.03)	(0.02)	(0.03)				
12-14	.3	5.78	6.49	0.17*	0.25*	0.17*	0.23	97	97	1003	901
				(0.03)	(0.05)	(0.03)	(0.05)				
15-18	.3	7.60	7.67	0.13*	0.28*	0.11*	0.22	96	95	595	477
				(0.04)	(0.09)	(0.04)	(0.08)				
19-20	.3	7.81	8.01	0.04	0.06	0.03	-.06	96	96	145	106
				(0.06)	(0.17)	(0.04)	(0.10)				
6-20	.3	4.09	4.25	0.11*	0.11*	0.12*	0.11	97	97	4470	3949
				(0.02)	(0.03)	(0.01)	(0.02)				
6-7	.4	0.69	0.90	0.05	-.01	0.06*	0.02	97	97	843	753
				(0.04)	(0.05)	(0.04)	(0.04)				
8-11	.4	3.08	3.44	0.11*	0.05	0.11*	0.06	97	97	1884	1712
				(0.02)	(0.03)	(0.02)	(0.03)				
12-14	.4	5.78	6.49	0.17*	0.26*	0.17*	0.23	97	97	1003	901
				(0.03)	(0.05)	(0.03)	(0.05)				
15-18	.4	7.60	7.67	0.13*	0.30*	0.11*	0.22	96	95	595	477
				(0.04)	(0.08)	(0.04)	(0.08)				
19-20	.4	7.81	8.01	0.03	0.03	0.03	-.06	96	96	145	106
				(0.05)	(0.13)	(0.04)	(0.10)				
6-20	.4	4.09	4.25	0.11*	0.12*	0.12*	0.11	97	97	4470	3949
				(0.02)	(0.02)	(0.01)	(0.02)				

† Difference-in-Difference estimator, imposing common support (2% trimming)
Standard errors based on bootstrap with 500 replications
*Significant at 10%
˜Percentage of observations in the support region for 1-year estimations
˜˜Percentage of observations in the support region for 2-year estimations

Table 4b

Estimated Impacts on Years of Schooling
Subgroup: Girls, Age 6-20
Local Linear matching †

Age in 2002	BW	Avg Trt in 2002	Avg Con in 2002	1 Year DID Match	2 Year DID Match	1 Year DID	2 Year DID	% Obs in Support ˜	% Obs in Support ˜˜	1 Year Obs T+C	2 Year Obs T+C
6-7	.2	0.73	0.88	0.09*	0.08*	0.11*	0.09	97	97	900	812
				(0.04)	(0.05)	(0.03)	(0.04)				
8-11	.2	3.11	3.50	0.13*	0.12*	0.13*	0.13	97	97	1930	1744
				(0.02)	(0.03)	(0.02)	(0.03)				
12-14	.2	5.87	6.48	0.14*	0.14*	0.14*	0.15	97	97	947	821
				(0.05)	(0.06)	(0.03)	(0.05)				
15-18	.2	7.54	8.19	0.07	0.17	0.05	0.16	96	97	583	446
				(0.05)	(0.11)	(0.04)	(0.08)				
19-20	.2	7.00	8.26	-.05	0.15	-.01	0.01	97	96	129	93
				(0.10)	(0.24)	(0.05)	(0.11)				
6-20	.2	3.98	4.29	0.11*	0.12*	0.11*	0.13	97	97	4489	3916
				(0.02)	(0.03)	(0.01)	(0.02)				
6-7	.3	0.73	0.88	0.10*	0.08*	0.11*	0.09	97	97	900	812
				(0.04)	(0.04)	(0.03)	(0.04)				
8-11	.3	3.11	3.50	0.12*	0.12*	0.13*	0.13	97	97	1930	1744
				(0.02)	(0.03)	(0.02)	(0.03)				
12-14	.3	5.87	6.48	0.14*	0.17*	0.14*	0.15	97	97	947	821
				(0.04)	(0.06)	(0.03)	(0.05)				
15-18	.3	7.54	8.19	0.07	0.19*	0.05	0.16	96	97	583	446
				(0.05)	(0.10)	(0.04)	(0.08)				
19-20	.3	7.00	8.26	0.00	-.06	-.01	0.01	97	96	129	93
				(0.08)	(0.19)	(0.05)	(0.11)				
6-20	.3	3.98	4.29	0.11*	0.13*	0.11*	0.13	97	97	4489	3916
				(0.02)	(0.02)	(0.01)	(0.02)				
6-7	.4	0.73	0.88	0.10*	0.08*	0.11*	0.09	97	97	900	812
				(0.04)	(0.04)	(0.03)	(0.04)				
8-11	.4	3.11	3.50	0.12*	0.12*	0.13*	0.13	97	97	1930	1744
				(0.02)	(0.03)	(0.02)	(0.03)				
12-14	.4	5.87	6.48	0.14*	0.17*	0.14*	0.15	97	97	947	821
				(0.04)	(0.05)	(0.03)	(0.05)				
15-18	.4	7.54	8.19	0.07	0.19*	0.05	0.16	96	97	583	446
				(0.05)	(0.09)	(0.04)	(0.08)				
19-20	.4	7.00	8.26	-.01	-.11	-.01	0.01	97	96	129	93
				(0.07)	(0.17)	(0.05)	(0.11)				
6-20	.4	3.98	4.29	0.11*	0.13*	0.11*	0.13	97	97	4489	3916
				(0.02)	(0.02)	(0.01)	(0.02)				

† Difference-in-Difference estimator, imposing common support (2% trimming)
Standard errors based on bootstrap with 500 replications
*Significant at 10%
˜Percentage of observations in the support region for 1-year estimations
˜˜Percentage of observations in the support region for 2-year estimations

Table 5a

Estimated Impacts on Percentage Enrolled in School
Subgroup: Boys, Age 6-20
Local Linear matching †

Age in 2002	BW	Avg Trt in 2002	Avg Con in 2002	1 Year DID Match	2 Year DID Match	1 Year DID	2 Year DID	% Obs in Support ˜	% Obs in Support ˜˜	1 Year Obs T+C	2 Year Obs T+C
6-7	.2	84	90	4.3	5.1*	5.1*	4.7	97	97	859	771
				(2.7)	(2.8)	(2.3)	(2.6)				
8-11	.2	95	98	2.5*	2.1*	2.3*	2.4	97	97	1947	1753
				(1.0)	(1.1)	(0.9)	(1.1)				
12-14	.2	93	90	1.6	-2.7	1.4	-1.8	97	97	1038	947
				(2.3)	(3.0)	(1.8)	(2.6)				
15-18	.2	60	53	7.1	3.8	5.6*	3.8	96	96	614	523
				(4.6)	(5.4)	(3.4)	(4.1)				
19-20	.2	22	13	18.9	-0.7	3.7	-4.9	96	97	146	120
				(13.2)	(10.8)	(5.1)	(5.5)				
6-20	.2	85	86	3.8*	1.5	3.1*	1.7	97	97	4604	4114
				(1.1)	(1.2)	(0.8)	(1.0)				
6-7	.3	84	90	4.7*	4.6	5.1*	4.7	97	97	859	771
				(2.6)	(2.8)	(2.3)	(2.6)				
8-11	.3	95	98	2.4*	2.2*	2.3*	2.4	97	97	1947	1753
				(0.9)	(1.1)	(0.9)	(1.1)				
12-14	.3	93	90	1.3	-3.0	1.4	-1.8	97	97	1038	947
				(2.0)	(3.1)	(1.8)	(2.6)				
15-18	.3	60	53	8.5*	2.6	5.6*	3.8	96	96	614	523
				(4.1)	(4.8)	(3.4)	(4.1)				
19-20	.3	22	13	10.2	-4.1	3.7	-4.9	96	97	146	120
				(9.3)	(8.1)	(5.1)	(5.5)				
6-20	.3	85	86	3.6*	1.2	3.1*	1.7	97	97	4604	4114
				(1.0)	(1.2)	(0.8)	(1.0)				
6-7	.4	84	90	4.6*	4.4	5.1*	4.7	97	97	859	771
				(2.5)	(2.8)	(2.3)	(2.6)				
8-11	.4	95	98	2.4*	2.2*	2.3*	2.4	97	97	1947	1753
				(0.9)	(1.1)	(0.9)	(1.1)				
12-14	.4	93	90	1.3	-2.8	1.4	-1.8	97	97	1038	947
				(2.0)	(3.0)	(1.8)	(2.6)				
15-18	.4	60	53	8.8*	3.6	5.6*	3.8	96	96	614	523
				(4.5)	(6.7)	(3.4)	(4.1)				
19-20	.4	22	13	5.8	-4.9	3.7	-4.9	96	97	146	120
				(9.8)	(8.2)	(5.1)	(5.5)				
6-20	.4	85	86	3.5*	1.3	3.1*	1.7	97	97	4604	4114
				(1.0)	(1.3)	(0.8)	(1.0)				

† Difference-in-Difference estimator, imposing common support (2% trimming)
Standard errors based on bootstrap with 500 replications
*Significant at 10%
˜Percentage of observations in the support region for 1-year estimations
˜˜Percentage of observations in the support region for 2-year estimations

Table 5b

Estimated Impacts on Percentage Enrolled in School
Subgroup: Girls, Age 6-20
Local Linear matching †

Age in 2002	BW	Avg Trt in 2002	Avg Con in 2002	1 Year DID Match	2 Year DID Match	1 Year DID	2 Year DID	% Obs in Support ˜	% Obs in Support ˜˜	1 Year Obs T+C	2 Year Obs T+C
6-7	.2	84	92	6.1*	7.9*	6.9*	9.1	97	97	927	835
				(2.6)	(2.8)	(2.1)	(2.2)				
8-11	.2	96	98	1.7*	2.6*	2.0*	1.9	97	97	1989	1796
				(0.9)	(1.5)	(0.8)	(1.2)				
12-14	.2	88	92	1.1	-0.3	4.1*	3.6	97	97	972	860
				(3.0)	(3.5)	(1.9)	(2.7)				
15-18	.2	58	61	4.4	1.3	2.6	4.8	97	97	609	486
				(4.9)	(6.9)	(3.8)	(4.6)				
19-20	.2	13	22	8.8	-9.1	4.7	-1.4	97	95	129	103
				(9.3)	(11.8)	(6.2)	(6.6)				
6-20	.2	84	89	3.0*	2.5*	3.6*	4.0	97	97	4626	4080
				(1.2)	(1.4)	(0.8)	(1.1)				
6-7	.3	84	92	6.0*	7.6*	6.9*	9.1	97	97	927	835
				(2.6)	(2.5)	(2.1)	(2.2)				
8-11	.3	96	98	1.7*	2.4*	2.0*	1.9	97	97	1989	1796
				(0.9)	(1.4)	(0.8)	(1.2)				
12-14	.3	88	92	1.0	0.1	4.1*	3.6	97	97	972	860
				(2.5)	(3.3)	(1.9)	(2.7)				
15-18	.3	58	61	3.1	1.2	2.6	4.8	97	97	609	486
				(4.7)	(6.2)	(3.8)	(4.6)				
19-20	.3	13	22	8.2	-2.1	4.7	-1.4	97	95	129	103
				(7.7)	(10.1)	(6.2)	(6.6)				
6-20	.3	84	89	2.8*	2.7*	3.6*	4.0	97	97	4626	4080
				(1.1)	(1.3)	(0.8)	(1.1)				
6-7	.4	84	92	6.3*	7.9*	6.9*	9.1	97	97	927	835
				(2.4)	(2.5)	(2.1)	(2.2)				
8-11	.4	96	98	1.7*	2.4*	2.0*	1.9	97	97	1989	1796
				(0.9)	(1.3)	(0.8)	(1.2)				
12-14	.4	88	92	1.3	0.5	4.1*	3.6	97	97	972	860
				(2.2)	(3.1)	(1.9)	(2.7)				
15-18	.4	58	61	2.7	0.8	2.6	4.8	97	97	609	486
				(4.5)	(5.5)	(3.8)	(4.6)				
19-20	.4	13	22	7.3	-6.3	4.7	-1.4	97	95	129	103
				(7.1)	(8.5)	(6.2)	(6.6)				
6-20	.4	84	89	2.8*	2.6*	3.6*	4.0	97	97	4626	4080
				(1.0)	(1.3)	(0.8)	(1.1)				

† Difference-in-Difference estimator, imposing common support (2% trimming)
Standard errors based on bootstrap with 500 replications
*Significant at 10%
˜Percentage of observations in the support region for 1-year estimations
˜˜Percentage of observations in the support region for 2-year estimations

Table 6a

Estimated Impacts on Percentage Helped with Homework, in school in 2001-2002
Subgroup: Boys, Age 6-18
Local Linear matching †

Age in 2002	BW	Avg Trt in 2002	Avg Con in 2002	1 Year DID Match	2 Year DID Match	1 Year DID	2 Year DID	% Obs in Support ˜	% Obs in Support ˜˜	1 Year Obs T+C	2 Year Obs T+C
6-7	.2	52	61	2.6	-8.5	6.3	-3.3	97	97	754	676
				(6.3)	(7.3)	(4.5)	(4.9)				
8-11	.2	52	49	-10.5*	-12.3*	-7.5*	-10.4	97	97	1880	1692
				(3.4)	(3.8)	(2.9)	(3.1)				
12-14	.2	30	33	2.1	2.0	2.0	2.7	97	97	950	870
				(4.2)	(4.1)	(3.6)	(3.7)				
15-18	.2	17	13	-4.6	-3.4	-4.9	-2.0	94	94	342	304
				(6.0)	(6.6)	(4.4)	(4.5)				
6-18	.2	43	46	-4.4*	-7.1*	-2.5	-4.9	97	97	3926	3542
				(2.5)	(2.6)	(2.0)	(1.9)				
6-7	.3	52	61	5.2	-7.3	6.3	-3.3	97	97	754	676
				(5.7)	(8.1)	(4.5)	(4.9)				
8-11	.3	52	49	-10.0*	-12.3*	-7.5*	-10.4	97	97	1880	1692
				(3.2)	(3.4)	(2.9)	(3.1)				
12-14	.3	30	33	2.1	2.0	2.0	2.7	97	97	950	870
				(4.2)	(4.0)	(3.6)	(3.7)				
15-18	.3	17	13	-4.7	-3.1	-4.9	-2.0	94	94	342	304
				(5.6)	(5.9)	(4.4)	(4.5)				
6-18	.3	43	46	-3.8	-6.8*	-2.5	-4.9	97	97	3926	3542
				(2.3)	(2.5)	(2.0)	(1.9)				
6-7	.4	52	61	5.9	-3.0	6.3	-3.3	97	97	754	676
				(5.2)	(5.4)	(4.5)	(4.9)				
8-11	.4	52	49	-9.7*	-12.1*	-7.5*	-10.4	97	97	1880	1692
				(3.2)	(3.4)	(2.9)	(3.1)				
12-14	.4	30	33	1.9	2.1	2.0	2.7	97	97	950	870
				(4.1)	(4.0)	(3.6)	(3.7)				
15-18	.4	17	13	-4.7	-3.3	-4.9	-2.0	94	94	342	304
				(5.0)	(6.0)	(4.4)	(4.5)				
6-18	.4	43	46	-3.6	-6.0*	-2.5	-4.9	97	97	3926	3542
				(2.2)	(2.2)	(2.0)	(1.9)				

† Difference-in-Difference estimator, imposing common support (2% trimming)
Standard errors based on bootstrap with 500 replications
*Significant at 10%
˜Percentage of observations in the support region for 1-year estimations
˜˜Percentage of observations in the support region for 2-year estimations

Table 6b

Estimated Impacts on Percentage Helped with Homework, in school in 2001-2002
Subgroup: Girls, Age 6-18
Local Linear matching †

Age in 2002	BW	Avg Trt in 2002	Avg Con in 2002	1 Year DID Match	2 Year DID Match	1 Year DID	2 Year DID	% Obs in Support ˜	% Obs in Support ˜˜	1 Year Obs T+C	2 Year Obs T+C
6-7	.2	57	58	-5.5	-6.1	-3.6	-5.9	97	97	818	739
				(4.9)	(5.5)	(4.3)	(5.0)				
8-11	.2	50	52	-5.4*	-6.9*	-4.1	-6.0	98	97	1931	1743
				(3.1)	(4.1)	(2.8)	(2.8)				
12-14	.2	26	29	1.7	-6.4	2.3	-5.2	97	97	876	783
				(4.4)	(4.9)	(3.7)	(3.9)				
15-18	.2	10	14	1.8	-0.0	3.0	-0.2	96	96	362	308
				(5.2)	(6.2)	(3.6)	(4.0)				
6-18	.2	42	45	-3.1	-6.0*	-1.9	-5.3	97	97	3987	3573
				(2.0)	(2.5)	(1.8)	(1.9)				
6-7	.3	57	58	-4.7	-5.2	-3.6	-5.9	97	97	818	739
				(4.7)	(5.3)	(4.3)	(5.0)				
8-11	.3	50	52	-5.3*	-6.5*	-4.1	-6.0	98	97	1931	1743
				(3.0)	(3.3)	(2.8)	(2.8)				
12-14	.3	26	29	2.3	-6.4	2.3	-5.2	97	97	876	783
				(4.5)	(4.7)	(3.7)	(3.9)				
15-18	.3	10	14	0.4	-2.9	3.0	-0.2	96	96	362	308
				(4.5)	(5.5)	(3.6)	(4.0)				
6-18	.3	42	45	-2.9	-5.9*	-1.9	-5.3	97	97	3987	3573
				(2.0)	(2.2)	(1.8)	(1.9)				
6-7	.4	57	58	-4.9	-5.2	-3.6	-5.9	97	97	818	739
				(4.5)	(5.2)	(4.3)	(5.0)				
8-11	.4	50	52	-5.3*	-6.1*	-4.1	-6.0	98	97	1931	1743
				(3.0)	(3.2)	(2.8)	(2.8)				
12-14	.4	26	29	1.6	-7.1	2.3	-5.2	97	97	876	783
				(4.1)	(4.5)	(3.7)	(3.9)				
15-18	.4	10	14	1.6	-3.3	3.0	-0.2	96	96	362	308
				(4.6)	(5.1)	(3.6)	(4.0)				
6-18	.4	42	45	-3.0	-5.9*	-1.9	-5.3	97	97	3987	3573
				(2.0)	(2.2)	(1.8)	(1.9)				

† Difference-in-Difference estimator, imposing common support (2% trimming)
Standard errors based on bootstrap with 500 replications
*Significant at 10%
˜Percentage of observations in the support region for 1-year estimations
˜˜Percentage of observations in the support region for 2-year estimations

Table 7a

Estimated Impacts on Hours Spent Doing Homework, in school in 2001-2002
Subgroup: Boys, Age 6-18
Local Linear matching †

Age in 2002	BW	Avg Trt in 2002	Avg Con in 2002	1 Year DID Match	2 Year DID Match	1 Year DID	2 Year DID	% Obs in Support ˜	% Obs in Support ˜˜	1 Year Obs T+C	2 Year Obs T+C
6-7	.2	4.36	4.69	0.92*	0.38	0.60*	0.03	97	97	754	676
				(0.43)	(0.52)	(0.36)	(0.38)				
8-11	.2	5.86	5.89	0.49*	0.31	0.41*	0.22	97	97	1880	1692
				(0.25)	(0.29)	(0.21)	(0.25)				
12-14	.2	6.26	6.40	1.30*	1.04*	0.96*	0.69	97	97	950	870
				(0.46)	(0.45)	(0.40)	(0.41)				
15-18	.2	7.86	6.46	1.34	-0.01	0.46	-0.60	96	96	257	228
				(1.17)	(1.16)	(0.88)	(0.82)				
6-18	.2	5.86	5.80	0.85*	0.50*	0.59*	0.25	97	97	3841	3466
				(0.20)	(0.23)	(0.16)	(0.19)				
6-7	.3	4.36	4.69	0.87*	0.42	0.60*	0.03	97	97	754	676
				(0.40)	(0.56)	(0.36)	(0.38)				
8-11	.3	5.86	5.89	0.51*	0.33	0.41*	0.22	97	97	1880	1692
				(0.23)	(0.29)	(0.21)	(0.25)				
12-14	.3	6.26	6.40	1.29*	1.08*	0.96*	0.69	97	97	950	870
				(0.43)	(0.44)	(0.40)	(0.41)				
15-18	.3	7.86	6.46	1.15	-0.07	0.46	-0.60	96	96	257	228
				(1.05)	(1.07)	(0.88)	(0.82)				
6-18	.3	5.86	5.80	0.83*	0.52*	0.59*	0.25	97	97	3841	3466
				(0.18)	(0.22)	(0.16)	(0.19)				
6-7	.4	4.36	4.69	0.86*	0.31	0.60*	0.03	97	97	754	676
				(0.38)	(0.42)	(0.36)	(0.38)				
8-11	.4	5.86	5.89	0.51*	0.34	0.41*	0.22	97	97	1880	1692
				(0.22)	(0.27)	(0.21)	(0.25)				
12-14	.4	6.26	6.40	1.30*	1.08*	0.96*	0.69	97	97	950	870
				(0.42)	(0.44)	(0.40)	(0.41)				
15-18	.4	7.86	6.46	1.10	-0.09	0.46	-0.60	96	96	257	228
				(0.96)	(0.96)	(0.88)	(0.82)				
6-18	.4	5.86	5.80	0.82*	0.50*	0.59*	0.25	97	97	3841	3466
				(0.17)	(0.20)	(0.16)	(0.19)				

† Difference-in-Difference estimator, imposing common support (2% trimming)
Standard errors based on bootstrap with 500 replications
*Significant at 10%
˜Percentage of observations in the support region for 1-year estimations
˜˜Percentage of observations in the support region for 2-year estimations

Table 7b

Estimated Impacts on Hours Spent Doing Homework, in school in 2001-2002
Subgroup: Girls, Age 6-18
Local Linear matching †

Age in 2002	BW	Avg Trt in 2002	Avg Con in 2002	1 Year DID Match	2 Year DID Match	1 Year DID	2 Year DID	% Obs in Support ˜	% Obs in Support ˜˜	1 Year Obs T+C	2 Year Obs T+C
6-7	.2	4.19	4.98	1.29*	0.46	1.17*	0.45	97	97	818	739
				(0.41)	(0.43)	(0.35)	(0.34)				
8-11	.2	5.83	5.80	0.38	0.36	0.15	0.04	98	97	1931	1743
				(0.27)	(0.27)	(0.23)	(0.23)				
12-14	.2	7.27	6.67	0.21	-0.74	0.03	-0.69	97	97	876	783
				(0.69)	(0.64)	(0.47)	(0.50)				
15-18	.2	8.53	7.56	-0.19	-0.78	0.12	0.04	97	97	258	217
				(1.75)	(1.44)	(1.08)	(1.00)				
6-18	.2	6.03	5.93	0.48*	0.05	0.33	-0.04	97	97	3883	3482
				(0.26)	(0.24)	(0.20)	(0.19)				
6-7	.3	4.19	4.98	1.38*	0.34	1.17*	0.45	97	97	818	739
				(0.40)	(0.41)	(0.35)	(0.34)				
8-11	.3	5.83	5.80	0.39	0.32	0.15	0.04	98	97	1931	1743
				(0.25)	(0.25)	(0.23)	(0.23)				
12-14	.3	7.27	6.67	0.19	-0.69	0.03	-0.69	97	97	876	783
				(0.78)	(0.64)	(0.47)	(0.50)				
15-18	.3	8.53	7.56	1.38	0.39	0.12	0.04	97	97	258	217
				(1.57)	(1.33)	(1.08)	(1.00)				
6-18	.3	6.03	5.93	0.61*	0.09	0.33	-0.04	97	97	3883	3482
				(0.27)	(0.23)	(0.20)	(0.19)				
6-7	.4	4.19	4.98	1.33*	0.33	1.17*	0.45	97	97	818	739
				(0.39)	(0.40)	(0.35)	(0.34)				
8-11	.4	5.83	5.80	0.39	0.29	0.15	0.04	98	97	1931	1743
				(0.25)	(0.24)	(0.23)	(0.23)				
12-14	.4	7.27	6.67	0.04	-0.57	0.03	-0.69	97	97	876	783
				(0.66)	(0.61)	(0.47)	(0.50)				
15-18	.4	8.53	7.56	0.76	0.47	0.12	0.04	97	97	258	217
				(1.32)	(1.21)	(1.08)	(1.00)				
6-18	.4	6.03	5.93	0.52*	0.11	0.33	-0.04	97	97	3883	3482
				(0.24)	(0.22)	(0.20)	(0.19)				

† Difference-in-Difference estimator, imposing common support (2% trimming)
Standard errors based on bootstrap with 500 replications
*Significant at 10%
˜Percentage of observations in the support region for 1-year estimations
˜˜Percentage of observations in the support region for 2-year estimations

Table 8a

Estimated Impacts on Percentage Employed
Subgroup: Boys, Age 12-20
Local Linear matching †

Age in 2002	BW	Avg Trt in 2002	Avg Con in 2002	1 Year DID Match	2 Year DID Match	1 Year DID	2 Year DID	% Obs in Support ˜	% Obs in Support ˜˜	1 Year Obs T+C	2 Year Obs T+C
12-14	.2	10	6	-7.9*	-13.2*	-6.9*	-13.4	97	97	938	857
				(2.8)	(3.6)	(2.4)	(3.0)				
15-18	.2	44	42	2.1	-2.6	0.6	-6.8	95	95	548	469
				(6.1)	(7.9)	(4.4)	(5.2)				
19-20	.2	81	78	-6.1	-2.2	3.1	-6.0	96	96	132	107
				(13.0)	(18.6)	(6.1)	(9.4)				
12-20	.2	28	24	-4.5	-9.1*	-3.6*	-10.8	96	96	1618	1433
				(2.8)	(3.5)	(2.1)	(2.5)				
12-14	.3	10	6	-7.7*	-12.4*	-6.9.	-13.4.	97	97	938	857
				(2.7)	(3.6)	(2.4)	(3.0)				
15-18	.3	44	42	2.8	-5.1	0.6.	-6.8.	95	95	548	469
				(5.9)	(8.2)	(4.4)	(5.2)				
19-20	.3	81	78	-5.9	-15.4	3.1.	-6.0.	96	96	132	107
				(9.9)	(15.2)	(6.1)	(9.4)				
12-20	.3	28	24	-4.1	-10.3*	-3.6.	-10.8.	96	96	1618	1433
				(2.6)	(3.6)	(2.1)	(2.5)				
12-14	.4	10	6	-7.7*	-12.4*	-6.9.	-13.4.	97	97	938	857
				(2.6)	(3.5)	(2.4)	(3.0)				
15-18	.4	44	42	3.9	-5.5	0.6.	-6.8.	95	95	548	469
				(5.7)	(6.6)	(4.4)	(5.2)				
19-20	.4	81	78	-3.8	-10.0	3.1.	-6.0.	96	96	132	107
				(7.9)	(13.7)	(6.1)	(9.4)				
12-20	.4	28	24	-3.6	-10.1*	-3.6.	-10.8.	96	96	1618	1433
				(2.6)	(3.0)	(2.1)	(2.5)				

† Difference-in-Difference estimator, imposing common support (2% trimming)
Standard errors based on bootstrap with 500 replications
*Significant at 10%
˜Percentage of observations in the support region for 1-year estimations
˜˜Percentage of observations in the support region for 2-year estimations

Table 8b

Estimated Impacts on Percentage Employed
Subgroup: Girls, Age 12-20
Local Linear matching †

Age in 2002	BW	Avg Trt in 2002	Avg Con in 2002	1 Year DID Match	2 Year DID Match	1 Year DID	2 Year DID	% Obs in Support ˜	% Obs in Support ˜˜	1 Year Obs T+C	2 Year Obs T+C
12-14	.2	10	5	0.9	-2.1	0.3	-1.1	97	97	886	787
				(4.4)	(3.3)	(1.9)	(2.4)				
15-18	.2	27	18	-11.3	-0.3	-5.8	0.2	97	97	562	452
				(7.1)	(7.5)	(4.1)	(5.0)				
19-20	.2	32	22	-3.5	11.8	-4.9	-0.3	95	96	116	92
				(9.2)	(29.4)	(7.5)	(9.9)				
12-20	.2	17	11	-3.8	-0.5	-2.3	-0.6	97	97	1564	1331
				(3.5)	(3.6)	(1.8)	(2.2)				
12-14	.3	10	5	0.7	-1.0	0.3.	-1.1.	97	97	886	787
				(2.6)	(2.8)	(1.9)	(2.4)				
15-18	.3	27	18	-10.5	0.4	-5.8.	0.2.	97	97	562	452
				(6.5)	(9.2)	(4.1)	(5.0)				
19-20	.3	32	22	-5.0	8.7	-4.9.	-0.3.	95	96	116	92
				(8.0)	(23.6)	(7.5)	(9.9)				
12-20	.3	17	11	-3.8	0.2	-2.3.	-0.6.	97	97	1564	1331
				(2.7)	(3.8)	(1.8)	(2.2)				
12-14	.4	10	5	0.4	-1.4	0.3.	-1.1.	97	97	886	787
				(2.4)	(2.7)	(1.9)	(2.4)				
15-18	.4	27	18	-10.4*	0.6	-5.8.	0.2.	97	97	562	452
				(5.4)	(7.4)	(4.1)	(5.0)				
19-20	.4	32	22	-3.4	2.5	-4.9.	-0.3.	95	96	116	92
				(7.0)	(21.2)	(7.5)	(9.9)				
12-20	.4	17	11	-3.8	-0.4	-2.3.	-0.6.	97	97	1564	1331
				(2.3)	(3.2)	(1.8)	(2.2)				

† Difference-in-Difference estimator, imposing common support (2% trimming)
Standard errors based on bootstrap with 500 replications
*Significant at 10%
˜Percentage of observations in the support region for 1-year estimations
˜˜Percentage of observations in the support region for 2-year estimations

Table 9a

Estimated Impacts on Monthly Income in Pesos
Subgroup: Boys, Age 12-20
Local Linear matching †

Age in 2002	BW	Avg Trt in 2002	Avg Con in 2002	1 Year DID Match	2 Year DID Match	1 Year DID	2 Year DID	% Obs in Support ˜	% Obs in Support ˜˜	1 Year Obs T+C	2 Year Obs T+C
12-14	.2	82	53	-62*	-54	-49*	-45	97	96	909	800
				(35)	(45)	(27)	(39)				
15-18	.2	454	452	12	105	12	65	95	96	435	369
				(132)	(146)	(75)	(101)				
19-20	.2	1151	1109	-591	-350	-128	-162	96	94	83	65
				(483)	(468)	(187)	(226)				
12-20	.2	261	239	-73	-24	-36	-19	96	96	1427	1234
				(55)	(58)	(31)	(41)				
12-14	.3	82	53	-66*	-47	-49*	-45	97	96	909	800
				(35)	(46)	(27)	(39)				
15-18	.3	454	452	32	183	12	65	95	96	435	369
				(119)	(168)	(75)	(101)				
19-20	.3	1151	1109	-537	-289	-128	-162	96	94	83	65
				(401)	(489)	(187)	(226)				
12-20	.3	261	239	-67	8	-36	-19	96	96	1427	1234
				(50)	(63)	(31)	(41)				
12-14	.4	82	53	-64*	-52	-49*	-45	97	96	909	800
				(34)	(46)	(27)	(39)				
15-18	.4	454	452	58	128	12	65	95	96	435	369
				(118)	(165)	(75)	(101)				
19-20	.4	1151	1109	-262	-206	-128	-162	96	94	83	65
				(473)	(417)	(187)	(226)				
12-20	.4	261	239	-41	-7	-36	-19	96	96	1427	1234
				(53)	(60)	(31)	(41)				

† Difference-in-Difference estimator, imposing common support (2% trimming)
Standard errors based on bootstrap with 500 replications
*Significant at 10%
˜Percentage of observations in the support region for 1-year estimations
˜˜Percentage of observations in the support region for 2-year estimations

Table 9b

Estimated Impacts on Monthly Income in Pesos
Subgroup: Girls, Age 12-20
Local Linear matching †

Age in 2002	BW	Avg Trt in 2002	Avg Con in 2002	1 Year DID Match	2 Year DID Match	1 Year DID	2 Year DID	% Obs in Support ˜	% Obs in Support ˜˜	1 Year Obs T+C	2 Year Obs T+C
12-14	.2	77	30	4	-12	-5	-7	97	97	871	775
				(49)	(38)	(22)	(30)				
15-18	.2	243	172	-114	-32	-53	-13	96	97	534	431
				(83)	(94)	(53)	(65)				
19-20	.2	349	274	58	221	25	33	96	96	106	88
				(133)	(509)	(107)	(201)				
12-20	.2	157	99	-34	-2	-20	-6	96	97	1511	1294
				(43)	(53)	(25)	(32)				
12-14	.3	77	30	3	3	-5	-7	97	97	871	775
				(28)	(33)	(22)	(30)				
15-18	.3	243	172	-100	-36	-53	-13	96	97	534	431
				(75)	(77)	(53)	(65)				
19-20	.3	349	274	4	121	25	33	96	96	106	88
				(115)	(452)	(107)	(201)				
12-20	.3	157	99	-34	-2	-20	-6	96	97	1511	1294
				(32)	(45)	(25)	(32)				
12-14	.4	77	30	-0	-2	-5	-7	97	97	871	775
				(27)	(32)	(22)	(30)				
15-18	.4	243	172	-98	-10	-53	-13	96	97	534	431
				(68)	(77)	(53)	(65)				
19-20	.4	349	274	46	54	25	33	96	96	106	88
				(122)	(405)	(107)	(201)				
12-20	.4	157	99	-32	-1	-20	-6	96	97	1511	1294
				(30)	(43)	(25)	(32)				

† Difference-in-Difference estimator, imposing common support (2% trimming)
Standard errors based on bootstrap with 500 replications
*Significant at 10%
˜Percentage of observations in the support region for 1-year estimations
˜˜Percentage of observations in the support region for 2-year estimations

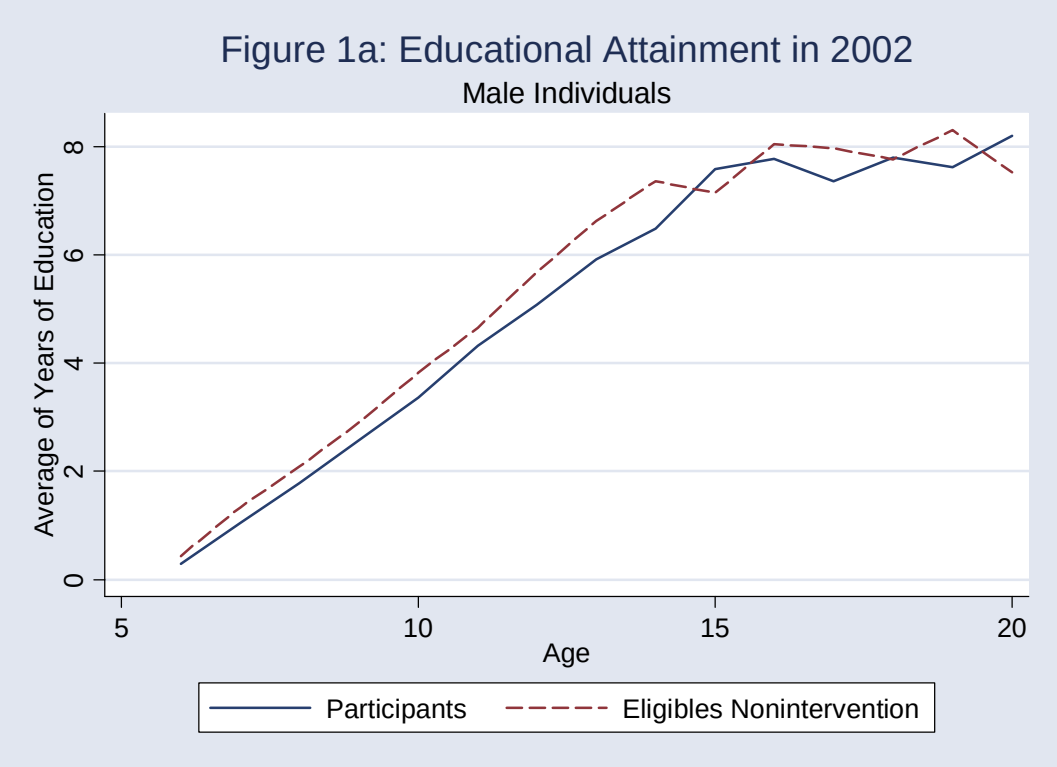
Figure 1a: Educational Attainment in 2002
Male Individuals
Average of Years of Education
0
2
4
6
8
5
10
15
20
Age
Participants
Eligibles Nonintervention

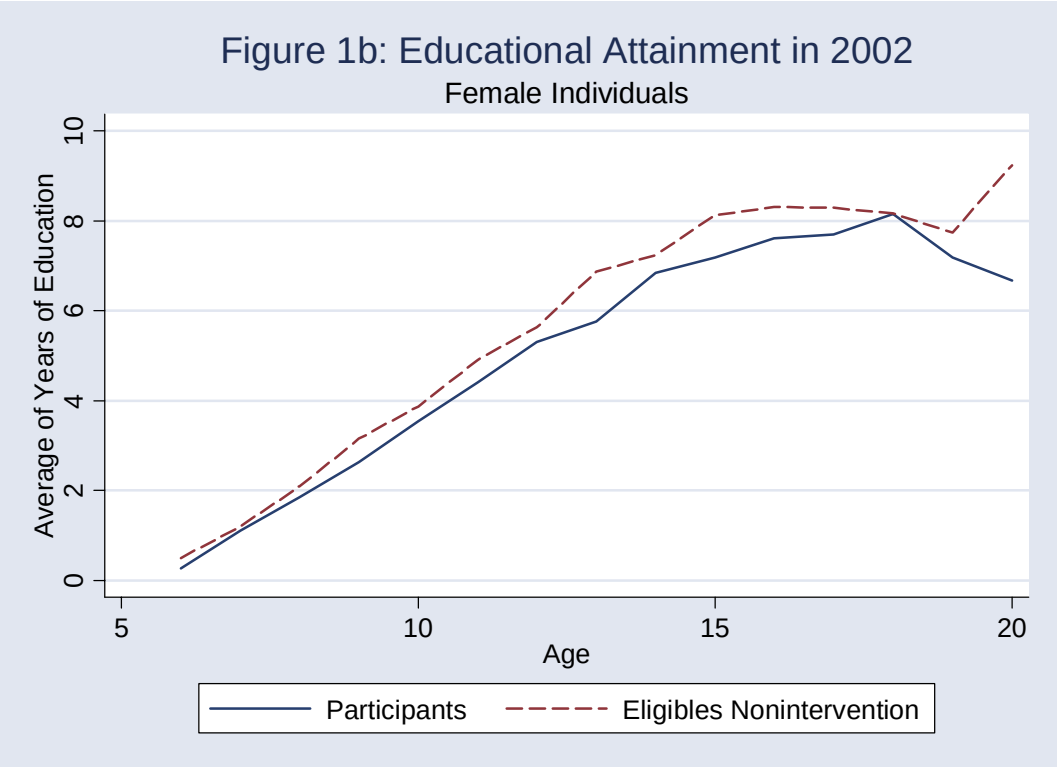
Figure 1b: Educational Attainment in 2002
Female Individuals
Average of Years of Education
0
2
4
6
8
10
5
10
15
20
Age
Participants
Eligibles Nonintervention

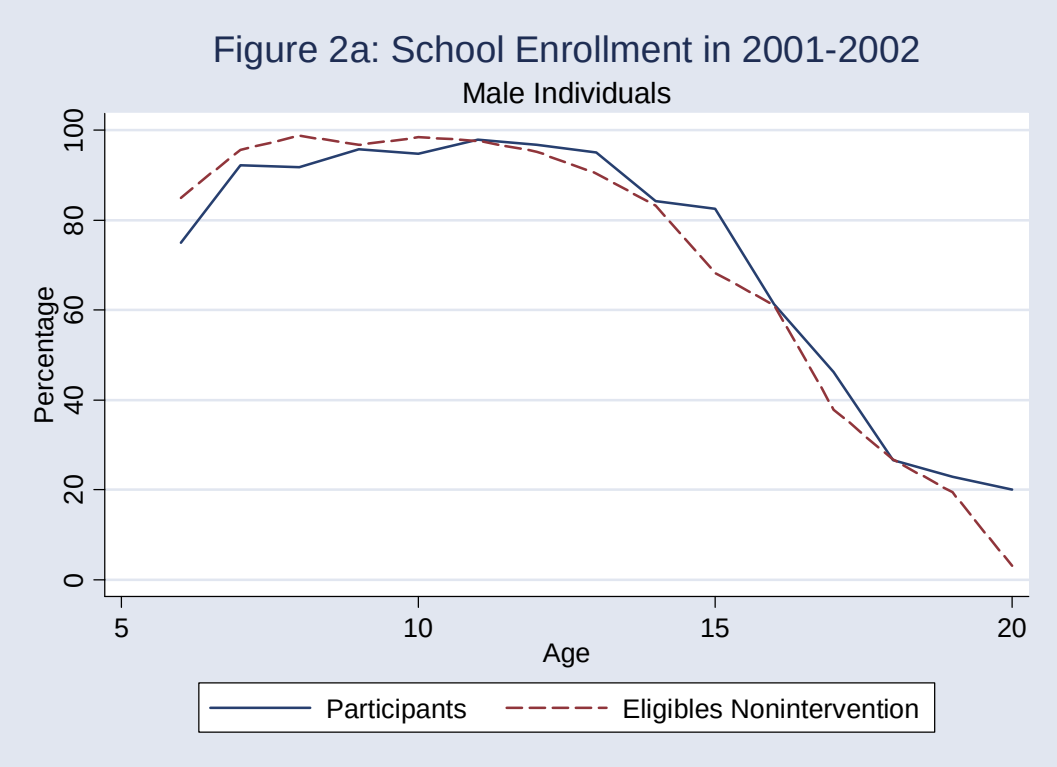
Figure 2a: School Enrollment in 2001-2002
Male Individuals
Percentage
0
20
40
60
80
100
5
10
15
20
Age
Participants
Eligibles Nonintervention

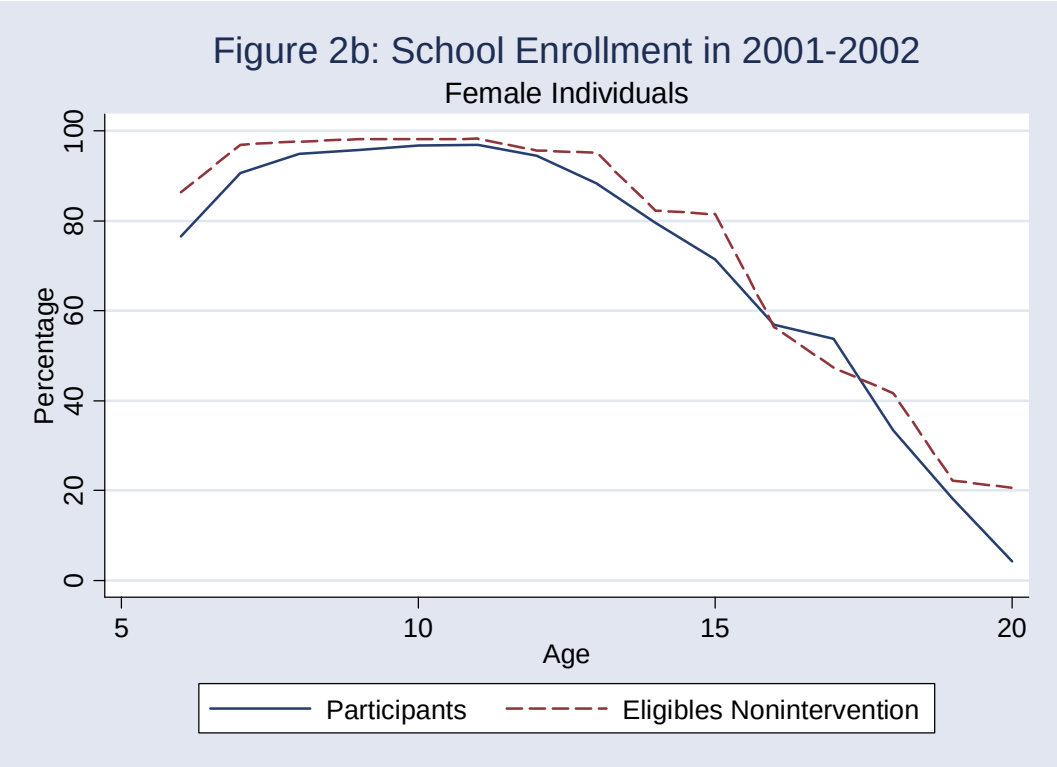
Figure 2b: School Enrollment in 2001-2002
Female Individuals
Percentage
0
20
40
60
80
100
5
10
15
20
Age
Participants
Eligibles Nonintervention

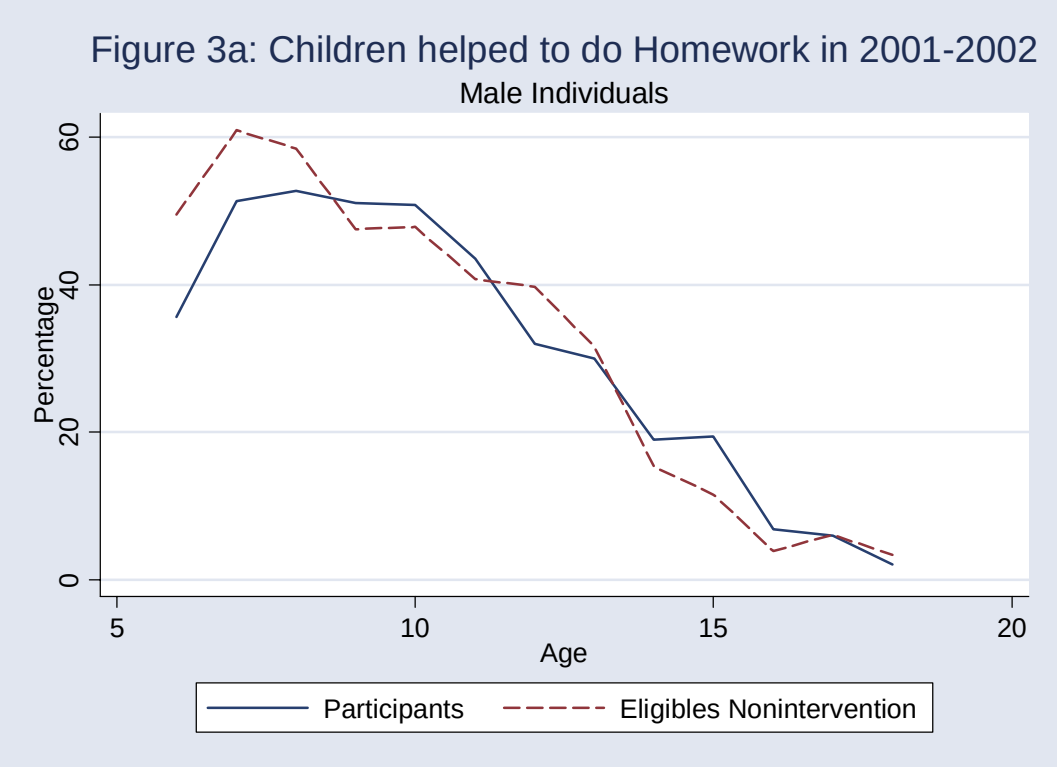
Figure 3a: Children helped to do Homework in 2001-2002
Male Individuals
Percentage
0
20
40
60
5
10
15
20
Age
Participants
Eligibles Nonintervention

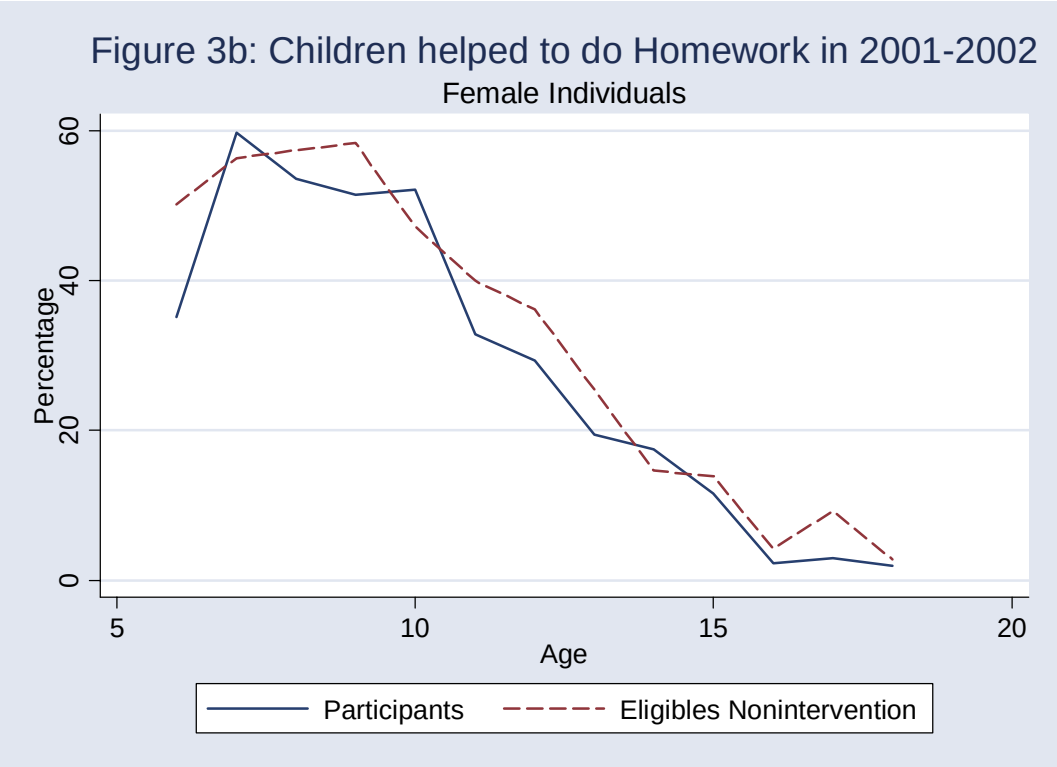
Figure 3b: Children helped to do Homework in 2001-2002
Female Individuals
Percentage
0
20
40
60
5
10
15
20
Age
Participants
Eligibles Nonintervention

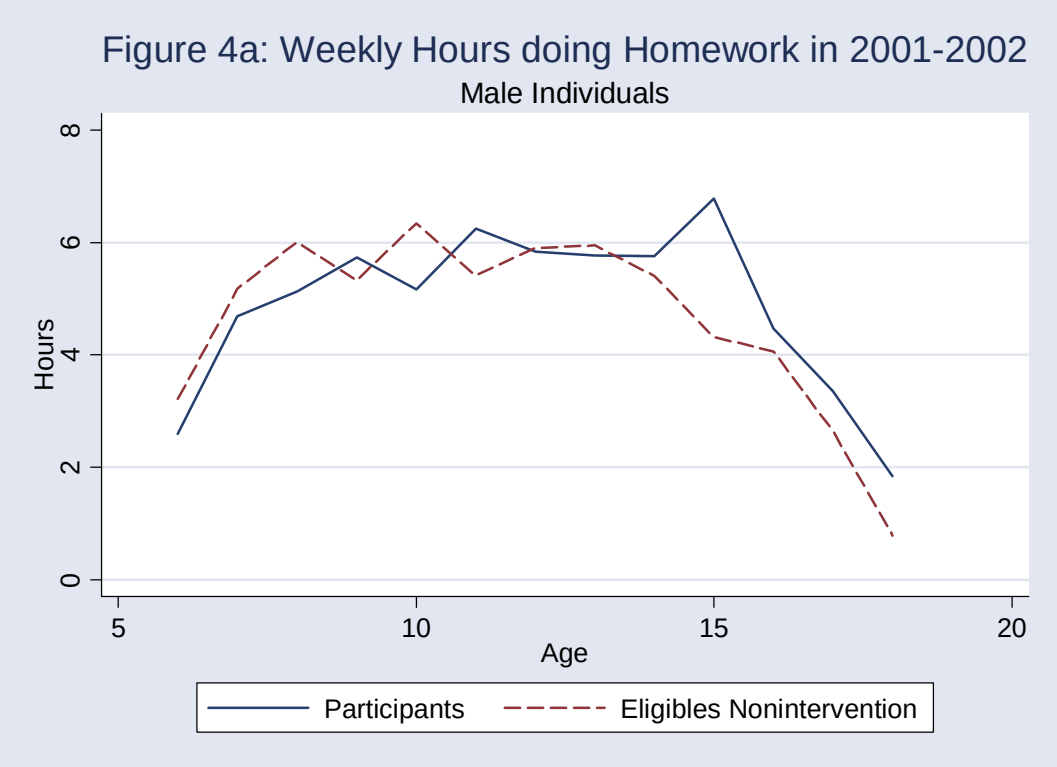
Figure 4a: Weekly Hours doing Homework in 2001-2002
Male Individuals
Hours
0
2
4
6
8
5
10
15
20
Age
Participants
Eligibles Nonintervention

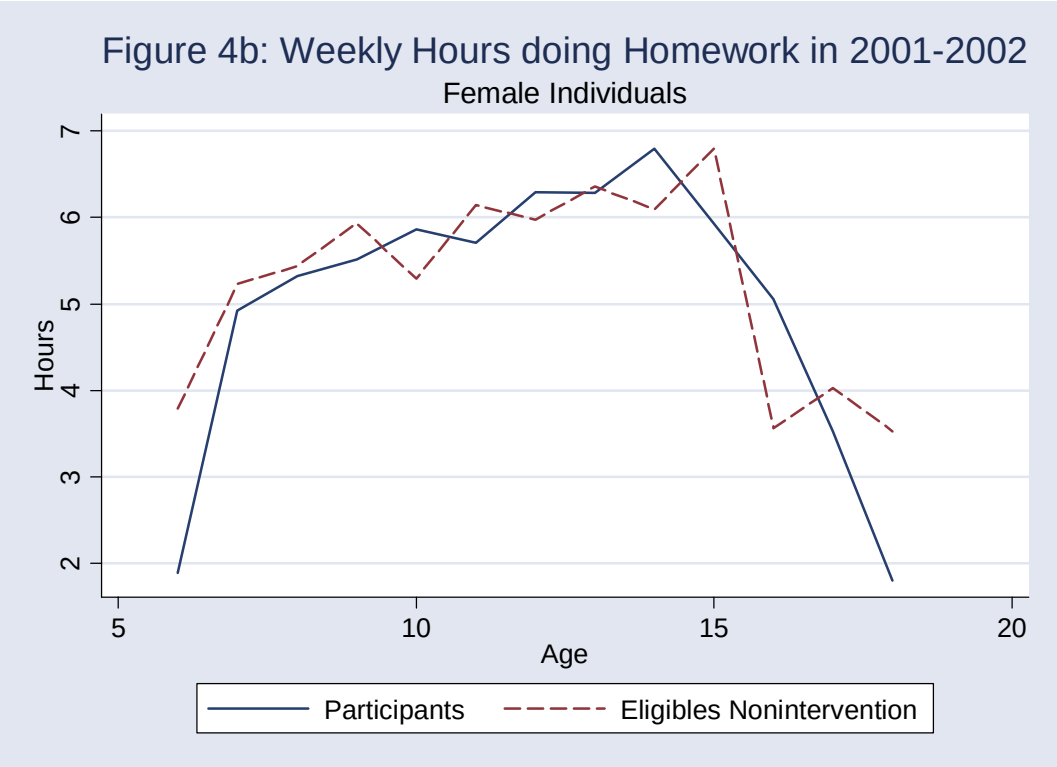
Figure 4b: Weekly Hours doing Homework in 2001-2002
Female Individuals
Hours
2
3
4
5
6
7
5
10
15
20
Age
Participants
Eligibles Nonintervention

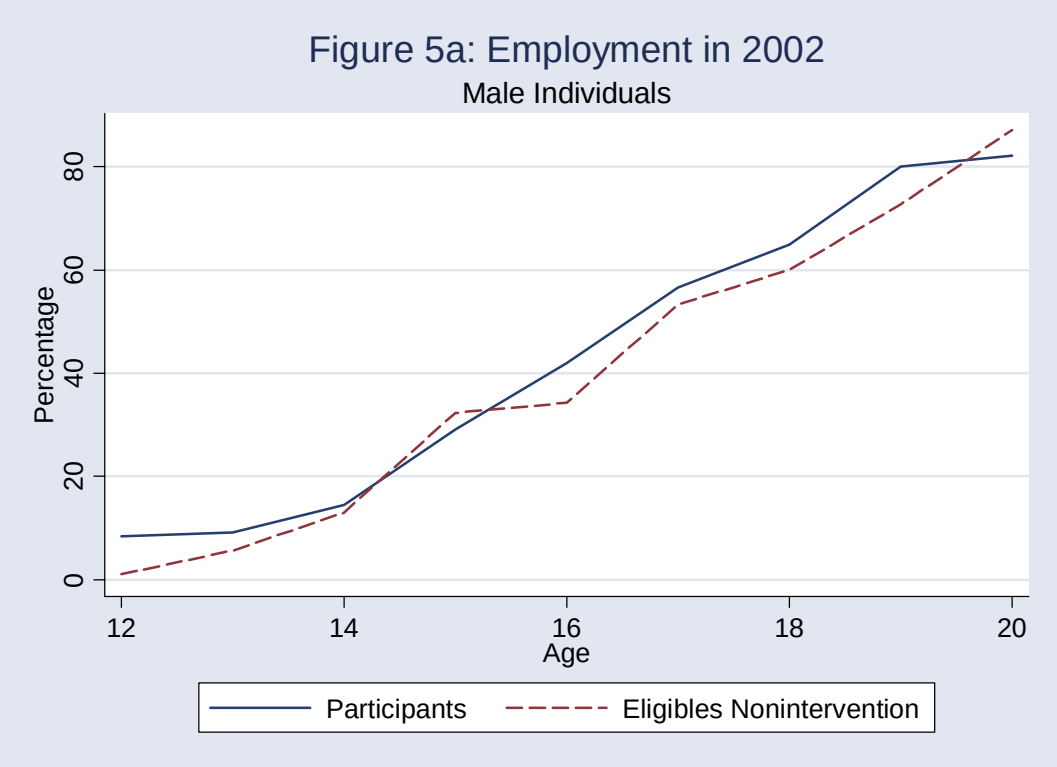
Figure 5a: Employment in 2002
Male Individuals
Percentage
0
20
40
60
80
12
14
16
18
20
Age
Participants
Eligibles Nonintervention

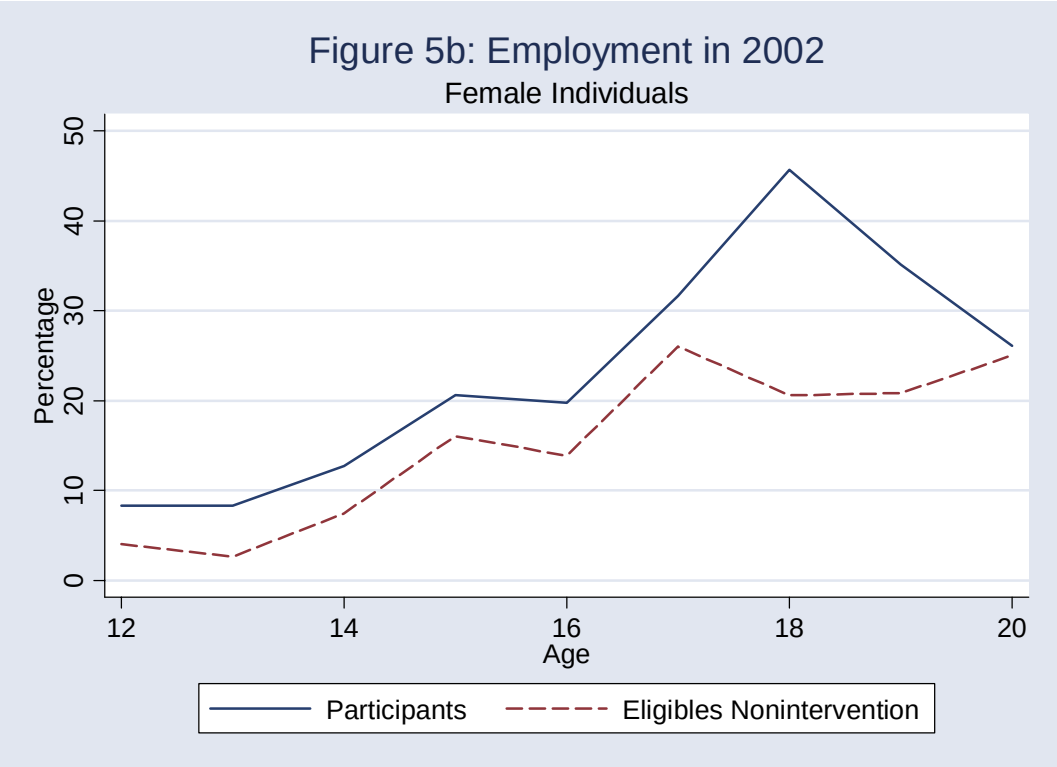
Figure 5b: Employment in 2002
Female Individuals
Percentage
0
10
20
30
40
50
12
14
16
18
20
Age
Participants
Eligibles Nonintervention

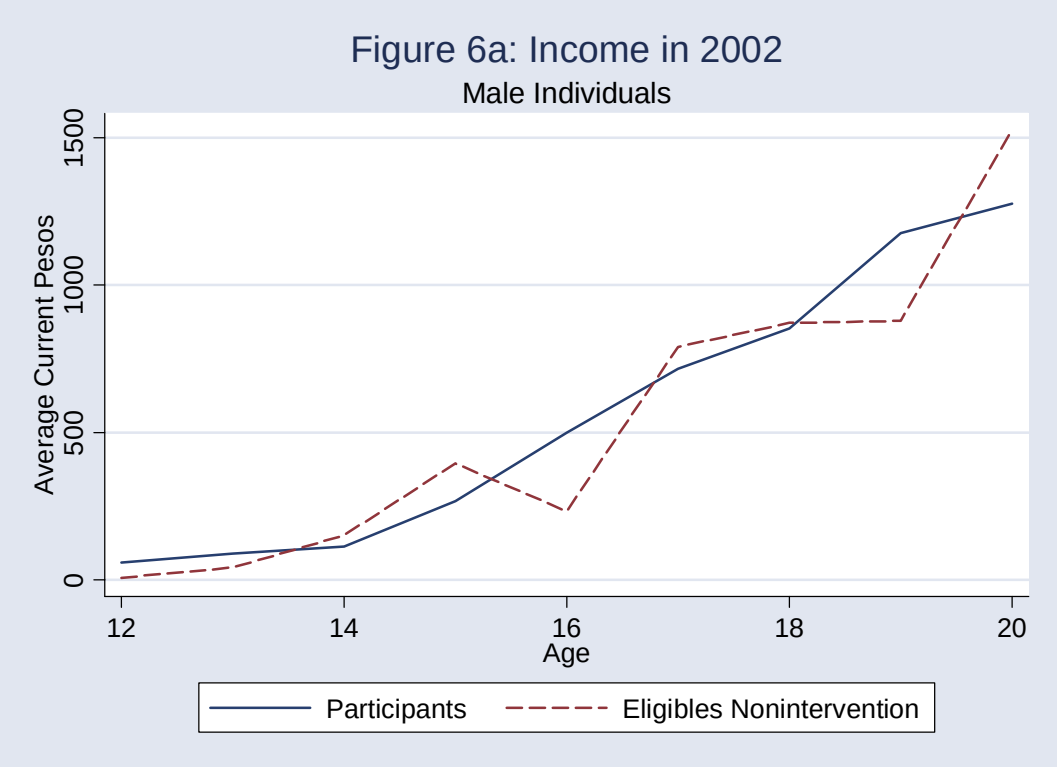
Figure 6a: Income in 2002
Male Individuals
Average Current Pesos
0
500
1000
1500
12
14
16
18
20
Age
Participants
Eligibles Nonintervention

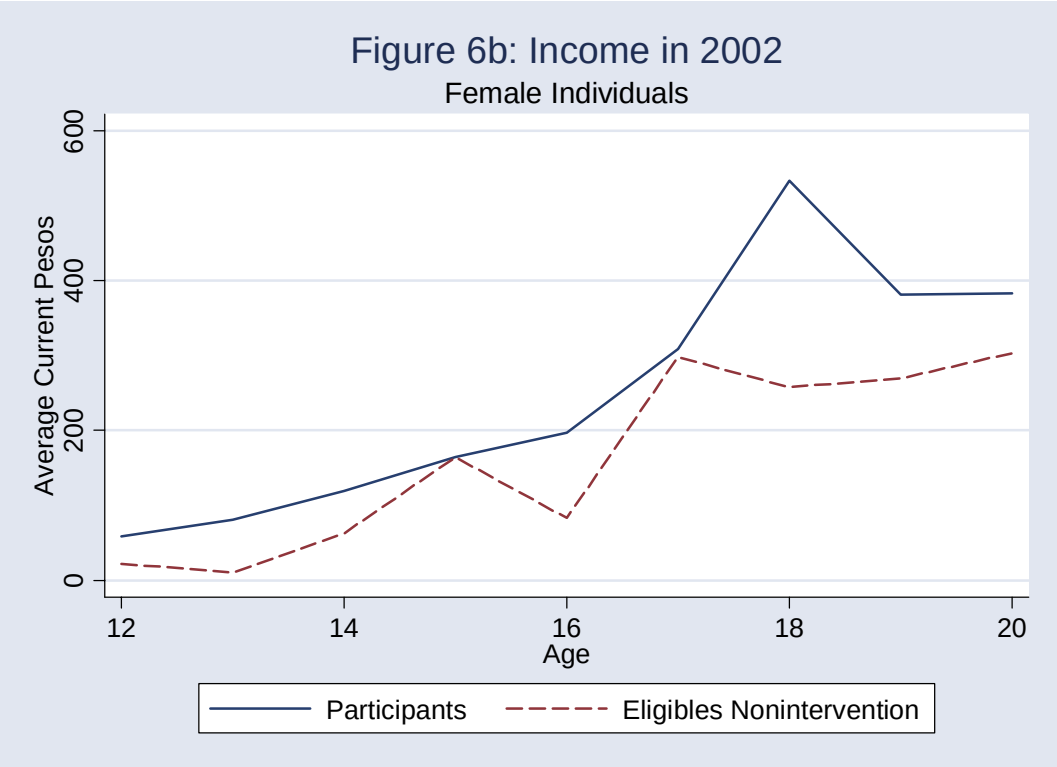
Figure 6b: Income in 2002
Female Individuals
Average Current Pesos
0
200
400
600
12
14
16
18
20
Age
Participants
Eligibles Nonintervention

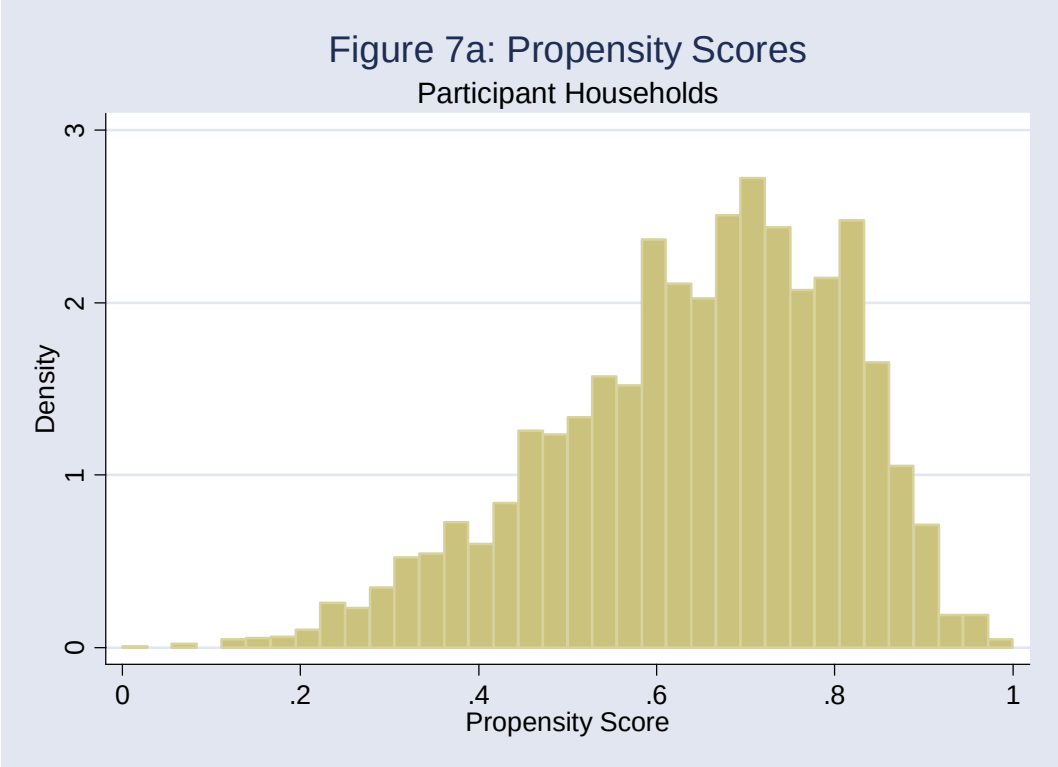
Figure 7a: Propensity Scores
Participant Households
Density
0
1
2
3
0
.2
.4
.6
.8
1
Propensity Score

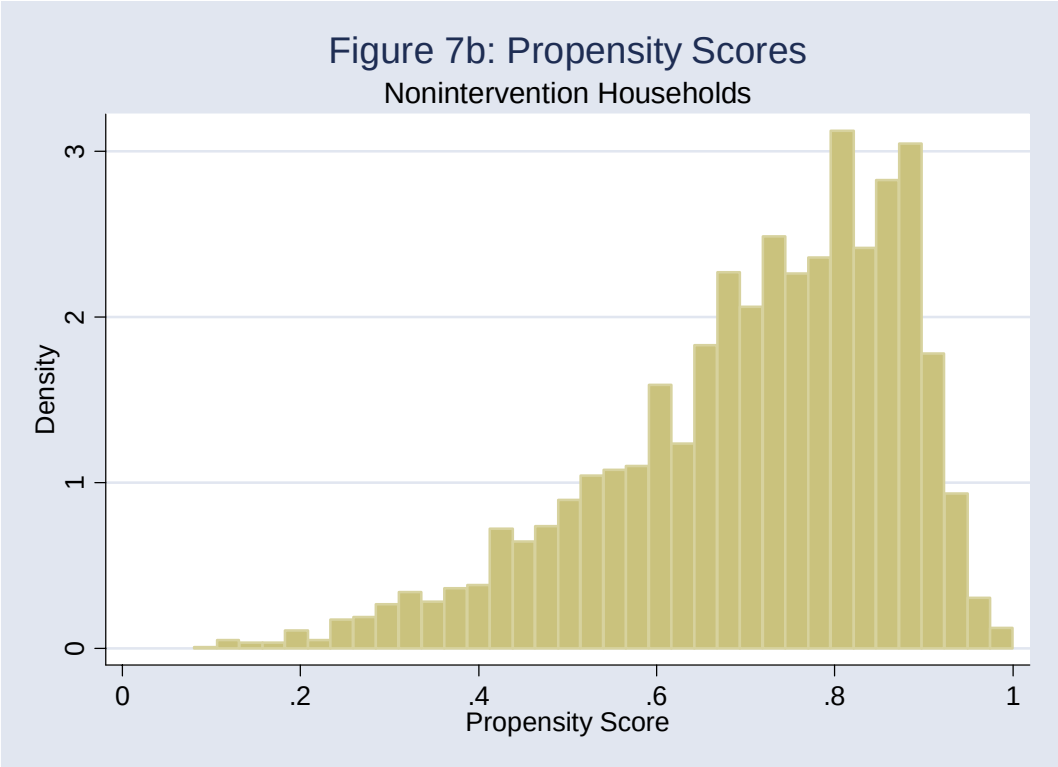
Figure 7b: Propensity Scores
Nonintervention Households
Density
0
1
2
3
0
.2
.4
.6
.8
1
Propensity Score

Table B.1
Definition of variables used in the propensity score model

Name Variable	Description
dirt floor	Equal to one if the house has dirt floor; equal to zero if the floor is made of hard materials such as cement, wood, or other coating materials or the information is missing.
floor missing	Equal to one if the floor material information is missing; zero otherwise.
wall material	Equal to one if the ceilings or the walls of the house are made of provisional materials such as cardboard, plastics, cloths, etc.; equal to zero otherwise.
mat missing	Equal to one if the ceilings and the walls material information is missing; equal to zero otherwise.
number rooms	Total number of rooms in the house other than those used as kitchen, bathrooms or hallways.
Room missng	Equal to one if the number of rooms' information is missing; equal to zero otherwise.
Hwater	Equal to one if there is water system inside the house; equal to zero otherwise.
Hwmiss	Equal to one if the water information is missing; equal to zero otherwise.
Have wc	Equal to one if the house has a WC, latrine, or toilet; equal to zero otherwise.
wc miss	Equal to one if the WC information is missing; equal to zero otherwise.
Owns house	Equal to one if the house is owned by a member of the family; equal to zero otherwise.
ownership missing	Equal to one if the possession of the house information is missing; equal to zero otherwise.
Rents house	Equal to one if the house is rented; equal to zero otherwise.
participates tortilla program	Equal to one if the household participates in the free tortilla program; equal to zero otherwise.
participates milk program	Equal to one if the household participates in the milk subsidies program; equal to zero otherwise.
participates diff	Equal to one if the household participates in the DIF (Integral Family Development) free groceries program; equal to zero otherwise.
participates breakf	Equal to one if the household participates in the free breakfast at school program; equal to zero otherwise.
participates educs	Equal to one if the household participates in scholarship programs other than Oportunidades; equal to zero otherwise.
participates trans	Equal to one if the household participates in the transportation grants program; equal to zero otherwise.
participates ini	Equal to one if the household participates in the INI (Indigenous National Institute) transfers program; equal to zero otherwise.
participates probecat	Equal to one if the household participates in the PROBECAT (Unemployed Training Grants Program); equal to zero otherwise.
participates agric	Equal to one if the household participates in the subsidies and transfers for the agriculture program; equal to zero otherwise.
participates house	Equal to one if the household participates in the housing subsidies program; equal to zero otherwise.
participates procampo	Equal to one if the household participates in the PROCAMPO (Program of Direct Rural Support); equal to zero otherwise.
participates wcred	Equal to one if the household participates in the personal credits with no

	collateral program; equal to zero otherwise.
participat1es fonaes	Equal to one if the household participates in the FONAES (National Support Fund for Organizations in Solidarity) program; equal to zero otherwise.
participates entrep	Equal to one if the household participates in the micro and small entrepreneurship support fund program; equal to zero otherwise.
Have car	Equal to one if a member of the household owns a car; equal to zero otherwise.
Have truck	Equal to one if a member of the household owns a truck; equal to zero otherwise.
Have refrigerator	Equal to one if the household has a refrigerator; equal to zero otherwise.
Have stove	Equal to one if the household has a stove; equal to zero otherwise.
Have washing machine	Equal to one if the household has a washing machine; equal to zero otherwise.
Have animal	Equal to one if the household has working or consumption animals; equal to zero otherwise.
employed	Equal to one if the head of the household is employed; equal to zero otherwise.
emp miss	Equal to one if the head of the household employment information is missing; equal to zero otherwise.
Partner employed	Equal to one if the partner of the head of the household is employed; equal to zero otherwise.
Partner emp miss	Equal to one if the partner of the head of the household employment information is missing; equal to zero otherwise.
Head self emp	Equal to one if the head of the household is self-employed; equal to zero otherwise.
hselfmiss	Equal to one if the head of the household self-employment information is missing; equal to zero otherwise.
Partner self emp	Equal to one if the partner of the head of the household is self-employed; equal to zero otherwise.
pselfmiss	Equal to one if the partner of the head of the household self-employment information is missing; equal to zero otherwise.
children age 0-5	Number of members of the household with ages 0 to 5.
children age 6-12	Number of members of the household with ages 6 to 12.
children age 13-15	Number of members of the household with ages 13 to 15.
children age 16-20	Number of members of the household with ages 16 to 20.
Tothh1	Equal to one if the total number of household members is equal to 1; equal to zero otherwise.
Tothh2	Equal to one if the total number of household members is equal to 2; equal to zero otherwise.
Tothh3	Equal to one if the total number of household members is equal to 3; equal to zero otherwise.
Tothh4	Equal to one if the total number of household members is equal to 4; equal to zero otherwise.
Tothh5	Equal to one if the total number of household members is equal to 5; equal to zero otherwise.
sgt60	Number of households members 60 or older.
Shpose	Number of extended family household members.
children 0-5 att sch last year	Number of member of the household with ages 0 to 5 that attended to school in 2001 school year.
children 6-12 att sch last year	Number of member of the household with ages 6 to 12 that attended to school in 2001 school year.
children 13-15 att sch last year	Number of member of the household with ages 13 to 15 that attended to school in 2001 school year.

1

Youth 16-20 att sch last year	Number of member of the household with ages 16 to 20 that attended to school in 2001 school year.
edu0	Equal to one if the maximum education of the household members is less than or equal to 5 grades of education; equal to zero otherwise.
edu1	Equal to one if the maximum education of the household members is equal to 6 grades of education; equal to zero otherwise.
edu2	Equal to one if the maximum education of the household members is greater than 6 and less than or equal to 9 grades of education; equal to zero otherwise.
edu3	Equal to one if the maximum education of the household members is greater than 9 and less than or equal to 12 grades of education; equal to zero otherwise.
edu4	Equal to one if the maximum education of the household members is greater than 12 grades of education; equal to zero otherwise.
Sfem	Equal to one if it is a female headed household; equal to zero otherwise.
Score_soc	Poverty score based on socio-economic household characteristics.
scoresq	Square of the poverty score based on socio-economic household characteristics.
Entidad 6,25,26,28	Equal to one if the household is in either of the following states: Coahuila, Sinaloa, Sonora, or Tamaulipas; equal to zero otherwise.
Entidad 11,16,24	Equal to one if the household is in either of the following states: Guanajuato, Michoacan, or San Luis Potosi; equal to zero otherwise.
Entidad 13,15,21,29	Equal to one if the household is in either of the following states: Hidalgo, Estado de Mexico, Puebla, or Tlaxcala; equal to zero otherwise.
Entidad 4	Equal to one if the household is in the state of Campeche; equal to zero otherwise.
Entidad 7	Equal to one if the household is in the state of Chiapas; equal to zero otherwise.
Entidad 12	Equal to one if the household is in the state of Guerrero; equal to zero otherwise.
Entidad 17	Equal to one if the household is in the state of Morelos; equal to zero otherwise.
Entidad 27	Equal to one if the household is in the state of Tabasco; equal to zero otherwise.
ate fish	Equal to one if last week fish was eaten in the household; equal to zero otherwise.
fish missing	Equal to one if fish information is missing; equal to zero otherwise.
ate porc	Equal to one if last week pork was eaten in the household; equal to zero otherwise.
porc missing	Equal to one if pork information is missing; equal to zero otherwise.
ate beef	Equal to one if last week beef was eaten in the household; equal to zero otherwise.
beef missing	Equal to one if beef information is missing; equal to zero otherwise.
ate chicken	Equal to one if last week chicken was eaten in the household; equal to zero otherwise.
chicken missing	Equal to one if chicken information is missing; equal to zero otherwise.
hus spend food	Equal to one if extra income for the husband is spent on food; equal to zero otherwise.
wife spend food	Equal to one if extra income for the wife is spent on food; equal to zero otherwise.
h spend shoe	Equal to one if extra income for the husband is spent on shoes; equal to zero otherwise.
w spend shoe	Equal to one if extra income for the wife is spent on shoes; equal to zero otherwise.
h spend furn	Equal to one if extra income for the husband is spent on furniture or household appliances; equal to zero otherwise.
w spend furn	Equal to one if extra income for the wife is spent on furniture or household appliances; equal to zero otherwise.
exhmiss	Equal to one if the information on extra income expenditure for the husband is

	missing; equal to zero otherwise.
exwmiss	Equal to one if the information on extra income expenditure for the wife is missing; equal to zero otherwise.
nprimpc	Number of primary schools per capita in the household community.
nmedspc	Number of preparatory schools per capita in the household community.
Nsalpc	Number of health clinics per capita in the household community.
Nsecpc	Number of secondary schools per capita in the household community.
Gdoofi	Average marginality index within the locality of the household.
Hgofi	Total number of households within locality.
hogares	Number of households in the household community that are eligible for the Oportunidades program (according to Census).
hog2	Square of the number of households in the household community that are eligible for the Oportunidades program (according to Census).
hog miss	Equal to one if the number of households in the household community that are eligible for the Oportunidades program is missing; equal to zero otherwise.
tamar7	Equal to one if the household locality has from 2500 to 14999 inhabitants; equal to zero otherwise.
tamar8	Equal to one if the household locality has from 15000 to 49999 inhabitants; equal to zero otherwise.
tamar9	Equal to one if the household locality has from 50000 to 74999 inhabitants; equal to zero otherwise.
tamar10	Equal to one if the household locality has from 75000 to 149999 inhabitants; equal to zero otherwise.
tamar11	Equal to one if the household locality has from 150000 to 299999 inhabitants; equal to zero otherwise.
tamar12	Equal to one if the household locality has 300,000 or more inhabitants; equal to zero otherwise.
Stotinc	Total household income
Stotinc2	Square of the total household income.
stot miss	Equal to one if the total household income is missing; equal to zero otherwise.

Table B.1

Model for Prob of Participation in Urban Oportunidades
D=1 - eligible poor in intervention areas enrolled in program
D=0 - eligible poor in intervention areas who did not apply

Variable	Coefficient	Std. Error	T-stat
intercept	-0.3849	1.0556	-0.36
dirt floor	0.2008	0.0813	2.47
floor missing	-0.7376	0.8392	-0.88
wall material	0.3549	0.0770	4.61
mat missing	0.3036	0.3287	0.92
number rooms	-0.1938	0.0482	-4.02
room missng	-1.3238	0.3536	-3.74
hwater	-0.0651	0.0838	-0.78
hwmiss	-0.0186	0.0774	-0.24
have wc	0.0072	0.0720	0.10
wc miss	1.5669	1.1526	1.36
owns house	0.1509	0.0759	1.99
ownership missing	0.3374	1.0577	0.32
rents house	-0.2416	0.1310	-1.84
participates tortilla program	0.2737	0.1040	2.63
participates milk program	0.3225	0.1230	2.62
participates diff	0.6654	0.1283	5.19
participates breakf	0.1352	0.1234	1.10
participates educs	0.2943	0.2293	1.28
participates trans	-0.1476	1.0262	-0.14
participates ini	-24.5098	624.5223	-0.04
participates probecat	1.0187	1.1340	0.90
participates agric	0.0143	1.4761	0.01
participates house	0.3032	0.5493	0.55
participates procampo	-0.8260	0.4061	-2.03
participates wcred	12.0133	521.5762	0.02
participates fonaes	-14.1897	1006.8292	-0.01
participates entrep	14.8335	739.3857	0.02
have car	0.1492	0.3729	0.40
have truck	-0.2849	0.3882	-0.73
have refrigerator	-0.2352	0.0882	-2.67
have stove	0.0926	0.0994	0.93
have washing machine	0.1947	0.1203	1.62
have animal	0.0107	0.0982	0.11
employed	0.2672	0.1014	2.63
emp miss	-0.6803	0.9325	-0.73
partner employed	0.0425	0.0799	0.53
partner emp miss	-0.3644	1.4443	-0.25
head self emp	0.0442	0.0773	0.57
hselfmiss	1.4077	0.8496	1.66
partner self emp	-0.1697	0.1823	-0.93
pselfmiss	0.0000	.	.
children age 0-5	0.0211	0.0519	0.41
children age 6-12	0.1056	0.0790	1.34

Table B.1 continued

Variable	Coefficient	Std. Error	T-stat
children age 13-15	-0.1881	0.0937	-2.01
children age 16-20	-0.0010	0.0511	-0.02
tothh1	-0.6205	0.3202	-1.94
tothh2	-0.3268	0.2010	-1.63
tothh3	-0.3506	0.1211	-2.89
tothh4	-0.1719	0.0913	-1.88
sgt60	0.1841	0.0720	2.56
shpose	0.3676	0.2248	1.64
children 0-5 att sch last year	0.0952	0.1249	0.76
children 6-12 att sch last year	0.1460	0.0704	2.07
children 13-15 att sch last year	0.4014	0.1050	3.82
youth 16-20 att sch last year	0.0159	0.0965	0.16
edu1	-0.1977	0.0946	-2.09
edu2	-0.3269	0.0911	-3.59
edu3	-0.4554	0.1256	-3.63
edu4	-0.8434	0.2335	-3.61
sfem	0.0297	0.0864	0.34
score_soc	0.6814	0.1922	3.54
scoresq	-0.1175	0.0416	-2.82
entidad 6,25,26,28	-0.8861	0.1965	-4.51
entidad 11,16,24	-0.3479	0.1687	-2.06
entidad 13,15,21,29	-0.1970	0.1243	-1.58
entidad 4	-1.0190	0.2174	-4.69
entidad 7	-0.0196	0.1217	-0.16
entidad 12	0.0109	0.2532	0.04
entidad 17	-0.9890	0.1492	-6.63
entidad 27	0.3695	0.2249	1.64
ate fish	-0.2781	0.0856	-3.25
ate porc	-0.1391	0.0778	-1.79
ate beef	-0.1132	0.0633	-1.79
ate chicken	-0.0779	0.0672	-1.16
fish missing	0.8853	0.9902	0.89
porc missing	-0.7259	1.3162	-0.55
beef missing	-0.3746	0.8457	-0.44
chicken missing	0.0135	1.2324	0.01
hus spend food	0.1983	0.1330	1.49
wife spend food	-0.1358	0.1122	-1.21
h spend shoe	0.1861	0.3061	0.61
w spend shoe	0.2810	0.2982	0.94
w spend furn	-0.5079	0.2554	-1.99
h spend furn	0.4900	0.2612	1.88
exhmiss	0.0210	0.1495	0.14
exwmiss	-0.0581	0.1327	-0.44
nprimpc	-25.4542	38.9609	-0.65
nmedspc	523.8866	158.6729	3.30
nsalpc	-207.6508	189.4557	-1.10
nsecpc	-288.3939	130.2432	-2.21
gdoofi	0.2694	0.0798	3.38

Table B.1 continued

Variable	Coefficient	Std. Error	T-stat
hgofi	-0.0000	0.0000	-0.12
hogares	0.0019	0.0010	1.99
hog2	-4.79e-6	1.62E-6	-2.96
hog miss	-0.8100	1.1520	-0.70
tamar7	-1.4226	0.9323	-1.53
tamar8	-1.0362	0.9219	-1.12
tamar9	-1.3266	0.9993	-1.33
tamar10	-1.1595	0.9062	-1.28
tamar11	-0.3890	0.8971	-0.43
tamar12	-1.1015	0.9141	-1.20
stotinc	-0.00004	0.000014	-2.85
stotinc2	4.53E-10	2.65E-10	1.71
stot miss	-0.3487	0.1141	3.85